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EDWIN DUNKIN, F.R.S., President, in the Chair.

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Lord McLaren, 46 Moray Place, Edinburgh;
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Harold Seward, B.A., Patent Office, Southampton Buildings, Chancery Lane;
Rev. Henry Wheaton, M.D., 9 Star Hill Terrace, Rochester; and
Rev. J. Wilkins, 16 Barforth Road, Peckham Rye, S.E.,

were balloted for and duly elected Fellows of the Society.

Note upon the Right Ascensions of certain Standard Polar Stars.
By Prof. Truman Henry Safford.

I should be glad to call the attention of observers in the Society to the need of more systematic observation of the stars of high northern declination. There are systematic discrepancies of a personal character between the Right Ascensions of stars near the Pole as determined by different observers, a fact which has been long recognised but little investigated, except quite lately; and the researches already made are largely unpublished. It would be desirable, I think, to insert in the principal star catalogues, as they are formed, more stars within a few degrees of the Pole than the number which would precisely correspond to the relative area of that region; or, in other

words, to extend the scheme of observation to fainter magnitude there than elsewhere. Practical reasons connected with geodetic astronomy make this also desirable.

A branch of philosophical study—physiological psychology—has lately been brought into relation with this subject by Professor Wundt, of Leipzig, and other investigators; and astronomers, especially in large observatories and in colleges, have the power, while contributing to their own science, to add valuable materials of observation to a far distant inquiry. The psycho-physical experiments proper will necessarily be less conclusive in certain directions than the astronomical, because the latter are the results of thoroughly trained professional ability, while the former will have more variety but be made with less fixity of habit.

The stars included in the following list are given in the American or the French Ephemerides, but are not in the Berlin *Jahrbuch* or the *Nautical Almanac*. Their use as standard polars is very often a great convenience, but is rendered difficult by the fact that their tabular places are derived from investigations made a good while ago, and are now uncertain or incorrect.

I have made a least-square determination of their proper motions from the best available material, and have, with the help of these proper motions, brought up the observations made since 1860. The tabular places corrected are for Nos. 1, 2, 5, those of the American *Ephemeris*, and for the remainder those of the *Annales du Bureau des Longitudes*, tome premier. The Ephemerides of 7 and 9, together with two stars also given in the Berlin *Jahrbuch*, are published in a small annual pamphlet by the Bureau des Longitudes, and those of the five others in the *Connaissance des Temps*.

No.	Star's Name.	Correction to Tabular Right Ascension. A.	B.	C.		Adopted Right Ascension. 1885·0.	Declin.
		s	s	s	s	h m s	° ′
1	Groom. 944	−0·12		−0·38	−0·011 (*t*−1885)	5 25 14·76	85 8
2	Camel. 25 H.	−0·44		−0·58	−0·008 (*t*−1885)	7 6 49·23	82 38
3	Groom. 1119	+6·16	+5·65	+6·1	+0·32 (*t*−1885)	7 41 4·71	88 58
4	Bradley 1672	+2·36	+1·87	+1·6	+0·063 (*t*−1885)	12 14 19·98	88 20
5	Camel. 32 H.	−0·42		−0·30	−0·009 (*t*−1885)	12 48 17·22	84 2
	Groom. 2283	−2·22	−2·32	−2·2	−0·089 (*t*−1885)	15 14 36·74	87 40
	Bradley 2701	+0·15	−0·30	+0·2	+0·008 (*t*−1885)	20 34 2·92	81 3
8	Groom. 3548	−0·71	−0·98	−0·76	−0·017 (*t*−1885)	21 22 23·71	86 34
9	Cephei 32 H.	+0·31	−0·25			22 22 18·48	85 32
	Cephei 39 H.	−0·08	−0·40			23 27 50·20	86 10

The corrections in column A are derived from my own observations for 1882–3, published in the *Proceedings of the American Academy* for 1884; those in column B from M. Gon-

nessiat's, at Lyons, in the *Comptes Rendus* for August 13, 1883; and those in column C from the combination of several determinations to which I have alluded. For stars 9 and 10 the correction was very small, and has been omitted. The corrections in one or two cases are quite sensible. The largest, for Groombridge 1119, is about $0^s.1$ when reduced to the parallel, and arises from a large error in Groombridge's Right Ascension. He seems, like all observers of his time, to have adjusted the instrument rather too rarely, so that it was probably $0^s.2$ or $3''$ of arc from its proper position in the region near the Pole when this star was observed; the error in his case is made absolutely certain by non-agreement of his position with a long series of others, especially Struve 1815, as well as by the increasing deviations from the positions of the *Connaissance des Temps*. The case of star No. 6 is similar. A revision of Groombridge's right ascensions in the immediate region of the Pole has long seemed to me a necessity. The instrumental corrections needed are probably quite as marked for his Meridian Circle as for the transit instruments at Greenwich in Maskelyne and Pond's time.

The comparison of M. Gonnessiat's Right Ascensions of close polars with my own, as given in my paper in the *Proceedings of the American Academy*, p. 347 of the last volume, shows that it is a little doubtful how to investigate and apply the peculiar personal equations which have been long recognised in this region. Not a very small part of them may, perhaps, be due to the cessation of the ordinary personal equation, if the eye-and-ear method be employed throughout. The observer is no longer liable to one kind of psychical disturbance, namely, that produced by the *rapidity* of the star's motion; and it may have been that peculiar anxiety which produced the large discrepancy between Bessel and Struve, for instance. There is no startling improbability in referring the whole constant difference between M. Gonnessiat and myself, about $0^s.4$ for the seven stars given here, to our different clock corrections, assuming that our methods of observing polars are identical.

Whatever be the form of personal equation here, there is no doubt that it would be interesting to follow it out; and I venture to request those astronomers who have well-mounted transits to pay some greater attention to the stars of this list, and any other close polars to the 7th or 8th magnitude.

Williamstown, Mass.:
1884, *Nov.* 27.

On Systematic Errors in the Readings of the Circle Microscopes of the Cape Transit-Circle. By David Gill, LL.D., F.R.S., Her Majesty's Astronomer at the Cape.

In the *Monthly Notices* for November 1876 Mr. Christie publishes an important paper "On the Effect of Wear in the Micrometer Screws of the Greenwich Transit-Circle."

The Cape Observations from 1871 to 1874 contain no reference to the existence of such errors in the Cape Transit-Circle; but, presumably by Mr. Christie's paper above referred to, the attention of Mr. Stone was directed to the question, for in January 1877 a series of Nadir observations was made on four days extending over the whole range of the screws, and in his Introductions to the Cape Observations for 1875 and 1876 Mr. Stone gives an example of the differences of Nadir, which he considers to be "about as large as the greatest difference obtained throughout the range of the screws employed in the observations." Mr. Stone further concludes that these errors are not due to wear, but that they existed when the screws were new, and were due to constraint, because the last two or three threads of the screw are not so highly finished as the rest.

I shall consider these conclusions of Mr. Stone at a further stage of the present paper.

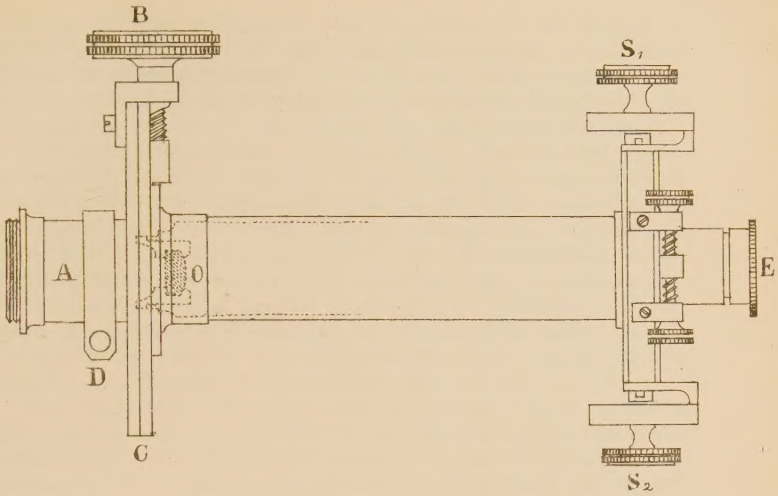
One of the first points, therefore, to which I turned my attention, after assuming direction of the Observatory in 1879, was to examine the condition of the screws of the Transit-Circle.

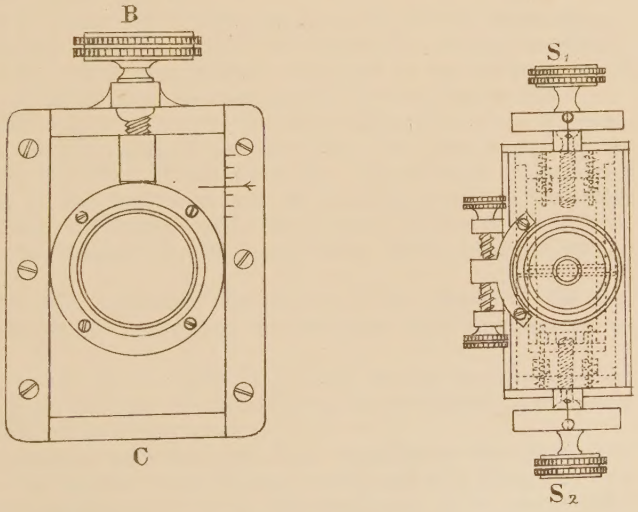
When the screws were properly cleaned and oiled there was no sign of constraint of the kind mentioned by Mr. Stone; the screws worked freely enough in their bushes throughout their whole range, though several of the slides had a tendency to stick towards the extremity of their range, from a cause that will be presently explained.

I thought it, however, best to have new screws with plenty of free, unconstrained range, and also to have an apparatus constructed by which the errors of the screws could be readily and minutely determined from time to time. The cause of the sticking of the slides appeared to be a defect in the design of the microscopes. The circular shoulders of the screws rested in hollow conical bearings that were *not* attached to the box of the micrometer, but to solid brass projections cast on the brass ring which is bolted to the pier, and upon which are also cast the hollow cylindrical projections to which the micrometer boxes are attached.

[The ring and projections in question are shown in section on Plate IX. of Sir George Airy's description of the Transit-Circle (Greenwich Observations 1852 and 1867) and in elevation on Plate VII.]

If the micrometer boxes had been rigidly attached to these hollow projections all might have been well, but the boxes were fixed to them by means of screws tapped into the ends of the





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hollow cylindrical projections, and the holes in the flanges of the micrometer boxes through which these screws passed were widened to allow adjustment of the boxes. Now, it is obvious that any shift of the micrometer box, except in the line of the axis of the screw, would at once derange the parallelism of the axis of the screw with its matrix and the slide. Such change would also shift the circle of bearing of the shoulder of the screw in the hollow cone, and might even cause the shoulder to bear on one side only of the hollow cone. The periodic errors of micrometer screws have origin more frequently in their bearings than in the thread of the screw; every time, therefore, that the micrometer box is removed for the purpose of cleaning or oiling the slide, it would be necessary to re-examine the errors of the screws, to say nothing of the risk of introducing a one-sided bearing of the shoulder, or a tendency in the slide to stick or jam.

The first thing, therefore, was to remove the micrometer boxes, and to have the bearings of the screw shoulders attached permanently to the ends of the boxes. This work was admirably executed by Mr. Simms, who also made an exquisite set of new screws. At the same time the old cross-wires by which the divisions were read were removed, and the more modern pattern of parallel wires, embracing the division, was introduced. The original eye-end was also sent home for repair (its draw-tube fits better than that of the new eye-end recently made), and was returned by Mr. Simms with new screws and in perfect working order.

For examining the errors of the screws I designed an apparatus of the following description, which has been admirably executed by Mr. Simms:

E O is a micrometer microscope, of which the object-glass is at O, the eye-piece at E, and the screws for moving its webs at S_1 and S_2. This microscope is attached to the micrometer (of which the screw is to be investigated) by the adapter A.

In the common focus of the object-glass O and the eye-piece E there are two pairs of parallel wires; one pair moved by the screw S_1, the other by the screw S_2. These wires are adjusted in focus of the objective, and to parallelism with the movable wire of the micrometer under examination, by means of a sliding tube in the adapter A, the latter being finally clamped by the clamp D.

With this apparatus the errors of the screws of the eye-end and of the microscope micrometers were investigated as follows:

By means of the slide B C, acted on by the screw B, the whole apparatus can be moved so as to command successively the whole range of the screw.

The screws were first tested for equality of the whole revolutions, the double wires (moved by S_1 and S_2) being set to a distance corresponding with one revolution of the micrometer screw under examination, and this distance was then measured in terms of successive revolutions of each micrometer screw.

The following table shows the excess of the length of each revolution of each screw over the length of a mean revolution of that screw. Each figure is the result of five or six sets of observations made by two or three different observers:

The screw-heads are divided into 100 parts.

	0r to 1r.	1r to 2r.	2r to 3r.	3r to 4r.	4r to 5r.
	r	r	r	r	r
Microscope A	−0·0013	+·0011	+·0009	−·0007	−·0001
„ B	− 5	+ 7	− 5	+ 1	+ 4
„ C	− 6	+ 4	+ 4	0	− 2
„ D	− 15	+ 9	+ 8	− 6	+ 3
„ E	− 6	− 3	− 5	+ 3	+ 9
„ F	− 13	− 10	+ 6	+ 12	+ 5
Mean ...	−0·0010	+·0003	+·0003	+·0001	+·0003

These errors were regarded as insignificant.

The periodic errors have been similarly investigated, employing a constant distance = about 0r·33.

The results are given in the following table:—

Measure beginning	r·0	r·1	r·2	r·3	r·4	r·5	r·6	r·7	r·8	r·9
A	−·0034	−·0034	−·0016	+·0010	+·0003	+·0003	+·0005	+·0009	+·0024	−·0004
B	− 4	− 10	− 14	+ 3	+ 5	+ 8	+ 8	− 6	+ 12	− 3
C	+ 38	+ 32	+ 26	− 15	− 32	− 36	− 23	− 9	+ 4	+ 15
D	− 3	− 2	+ 8	+ 9	+ 5	− 9	+ 9	− 5	− 8	− 4
E	− 15	− 16	− 19	− 3	+ 10	+ 11	+ 17	+ 17	+ 2	− 4
F	− 15	− 16	− 2	+ 7	+ 16	+ 20	+ 9	+ 3	− 11	− 13

The corrections to be applied to the readings of each screw-head were then computed from the above determinations by Bessel's method. The resulting corrections are:

For Screw A	−·00125 cos *u*	+·00022 sin *u*	−·00010 cos 2*u*	−·00108 sin 2*u*
„ B	− 35	− 20	+ 39	− 35
„ C	+ 186	− 83	− 28	+ 36
„ D	+ 5	+ 34	− 16	00
„ E	− 103	− 7	+ 4	− 5
„ F	− 77	+ 66	+ 11	− 3

The corresponding corrections in arc are:

	r·0	r·1	r·2	r·3	r·4	r·5	r·6	r·7	r·8	r·9
	"	"	"	"	"	"	"	"	"	"
A	−0·084	−·121	−·046	+·082	+·133	+·072	−·011	−·024	+·007	−·009
B	+·002	−·038	−·052	−·012	−·038	+·046	+·012	−·014	−·002	+·018
C	+·098	+·078	+·014	−·082	−·151	−·133	−·090	+·041	+·086	+·097
D	−·007	+·012	+·029	+·027	+·007	−·013	−·018	−·013	−·011	−·013
E	−·062	−·057	−·028	+·016	+·053	+·067	+·052	+·021	−·016	−·045
F	−·041	−·011	+·024	+·056	+·070	+·055	+·011	−·037	−·065	−·063
Mean	−0·016	−·023	−·010	+·015	+·025	+·016	−·007	−·004	·000	−·003

These corrections compensate each other so perfectly in the mean that, if the readings of all the micrometer heads are nearly the same, the maximum error of the mean will be $\pm 0''\cdot025$. The eccentricity of the circle has been determined with great care and found to be $2''\cdot20$, and the distances between the microscopes are almost exactly equal, so that the readings of the microscopes are never very different in practice.

In practice the Declination Micrometer Screw is used only over about four revolutions. The normal setting is 30^r, and the observers are instructed to clamp the instrument so that the star under observation shall transit within $\pm 2^r$ of this reading. The examination of the screw was therefore confined to the six central revolutions, viz. 27^r to 33^r.

A space equal to about two revolutions of the Declination Micrometer Screw was set off on the screw-testing apparatus, and measured with the following results:

27^r to 29^r.	29^r to 31^r.	31^r to 33^r.
r	r	r
$-\cdot0001$	$-\cdot0006$	$+\cdot0007$

The space 27^r to 30^r was also compared with 30^r to 33^r with the following results:

27^r to 30^r.	30^r to 33^r.
r	r
$+\cdot0010$	$-\cdot0010$

The screw may therefore be considered practically perfect as to equality in the whole revolutions.

The periodic errors were then investigated precisely in the same manner as those of the Circle Microscopes, and found to be, from six sets of measures of a space $= 0^r\cdot33$:

$$+\cdot00073\cos u - \cdot00012\sin u - \cdot00089\cos 2u - \cdot00008\sin 2u.$$

The value of one revolution of the screw is $28''\cdot580$, so that the corrections to be applied to the screw readings expressed in arc are:

$r\cdot0$	$r\cdot1$	$r\cdot2$	$r\cdot3$	$r\cdot4$	$r\cdot5$	$r\cdot6$	$r\cdot7$	$r\cdot8$	$r\cdot9$
$''$	$''$	$''$	$''$	$''$	$''$	$''$	$''$	$''$	$''$
$-\cdot004$	$+\cdot005$	$+\cdot022$	$+\cdot012$	$-\cdot024$	$-\cdot046$	$-\cdot025$	$+\cdot015$	$+\cdot031$	$+\cdot013$

These errors I considered too insignificant for application, seeing that the same star will generally be bisected by different parts of the screw at each observed culmination.

All these investigations were made in March 1880, and it seemed that all the screws might be regarded as perfect within limits of error far less than $0''\cdot1$. But on my return from England in September of the present year, and in examining the results of work done in my absence, I observed a marked variation in the run of the microscopes, which seemed to depend

on the part of the microscope screws employed in the determination.

To investigate this point the following determinations of run were made, each result being the mean of four determinations (extending over the whole range of the screws on each of two different days) by each of two observers.

As a matter of convenience the usual notation of the readings is increased by 5^r, so that in the following paper the readings commonly recorded 0^r and 5^r will be denoted 5^r and 10^r respectively.

The runs of each microscope were thus found to be the following :

TABLE I.

Lower reading of Mic. A.	Equivalents of 5′ in terms of each Micrometer Screw.						
	A	B	C	D	E	F	Mean.
	r	r	r	r	r	r	r
2·0	4·7920	4·8128	4·8128	4·7765	4·8179	4·7920	4·8007
2·33	·7890	·8135	·8146	·7811	·8140	·7944	·8011
2·67	·7926	·8136	·8065	·7791	·8174	·7970	·8010
3·0	·7920	·8109	·8086	·7786	·8191	·7967	·8010
3·33	·7935	·8129	·8118	·7851	·8188	·7973	·8032
3·67	·7979	·8105	·8036	·7848	·8217	·7974	·8027
4·0	·7993	·8094	·8065	·7875	·8231	·8013	·8045
4·33	·8000	·8087	·8116	·7949	·8233	·8016	·8067
4·67	·8069	·8137	·8060	·7986	·8272	·8090	·8102
5·00	·8196	·8139	·8106	·8106	·8305	·8127	·8163
5·33	·8292	·8207	·8176	·8316	·8380	·8206	·8263
5·67	·8376	·8264	·8193	·8470	·8457	·8255	·8336
6·00	·8433	·8251	·8195	·8533	·8486	·8298	·8366
6·33	·8401	·8285	·8242	·8601	·8485	·8280	·8382
6·67	·8457	·8286	·8248	·8656	·8508	·8334	·8415

These results are corrected for the small periodic errors of the screws.

The probable error of each of these results for the separate microscopes, as deduced from the agreement of the eight separate sets, is $\pm 0^r{\cdot}00098$, so that we may consider each figure in the mean result to represent the mean of the screw readings with a probable error of $\pm 0^r{\cdot}0004$. There can be no doubt, therefore, as to the systematic character of the variation of the runs, not only in the mean, but for each separate microscope. The law of this variation is also of the same general character, although the actual amount of variation is very different in the different microscopes. I could hardly believe it possible that

errors of such an extent had been produced by wear in little more than four years; for if such errors were produced in four years, to what would they amount in twenty-four years?—that is, from the erection of the Transit-Circle in 1855 till the new screws were made in 1879. I began to suspect that the errors were produced by optical distortion of the field of the microscopes.

The observed variations in run depend on the *difference* of the errors at the upper and lower readings of the screws, and in order to arrive at an approximate knowledge of their character I assumed that the errors depending on the reading of the mean screw could be represented by the series:

$$\text{True reading} = n + an^2 + bn^3 + \&c. \quad . \quad . \quad .$$

This series had to be carried to the fifth power of n before any reasonable representation of the observations was arrived at. The correction to be applied to the mean reading of the six microscopes was then found to be:

$$35^{r}\cdot56n - 22^{r}\cdot91n^2 - 6^{r}\cdot162n^3 + 0^{r}\cdot636n^4 + 0^{r}\cdot1788n^5,$$

the unit being $\frac{1}{10000}$ of a revolution, and n being reckoned from $7^{r}\cdot0$. With this expression the mean observations of Table I. are represented as follows:

Screw Readings.		Observed Excess of Mean.	Computed Excess of Mean.	C−O.
r	r	r	r	r
2·0	and 6·8	+0·0142	+0·0136	−·0006
2·33	7·13	+ 138	+ 136	− 2
2·67	7·47	+ 139	+ 144	+ 5
3·0	7·80	+ 139	+ 143	+ 4
3·33	8·13	+ 117	+ 141	+ 24
3·67	8·47	+ 122	+ 128	+ 6
4·00	8·80	+ 104	+ 102	− 2
4·33	9·13	+ 82	+ 65	− 17
4·67	9·47	+ 47	+ 16	− 31
5·00	9·80	− 14	− 37	− 23
5·33	10·13	− 144	− 94	− 20
5·67	10·47	− 187	− 155	+ 32
6·0	10·80	− 217	− 208	+ 9
6·33	11·13	− 233	− 247	− 14
6·67	11·47	− 266	− 267	− 1

It is obvious from these residuals that to obtain a representation of the observations within limits of their probable errors

it would be necessary to include terms to the seventh power of n. The solution is, however, sufficiently exact as it stands for the purpose of determining the general character of the errors and of tracing their probable source.

The corrections to be applied to the screw readings according to this solution are:

Screw Reading.	Correction.	Screw Reading.	Correction.
r	r	r	r
2·0	−·0143	7·0	±·0000
2·5	− 134	7·5	+ 12
3·0	− 134	8·0	+ 7
3·5	− 139	8·5	− 14
4·0	− 139	9·0	− 54
4·5	− 129	9·5	− 108
5·0	− 109	10·0	− 171
5·5	− 82	10·5	− 232
6·0	− 52	11·0	− 273
6·5	− 23	11·5	− 273

We may now obtain a pretty accurate notion of the cause of these errors. The corrections are practically the same from $2^r{\cdot}0$ to $4^r{\cdot}5$ where the screw is never used; they attain a maximum at $7^r{\cdot}5$ where the screw is most used; and they again become uniform after $10^r{\cdot}5$, which is beyond the part of the screw used in regular work. All this points to *wear* as the source of the errors in question; let us see if the sign of these corrections corresponds with what it would be in case of wear. Suppose that the screw readings increase as the spiral spring of the slide is compressed. If, then, the screw thread is worn at any part, the slide will not be so far moved against the spring for the same reading of the head as it would have been if the screw thread were not worn; or to make the bisection of a division we have to move the head farther (i.e. to a greater reading) than if the screw were not worn; the correction on account of wear would therefore be negative. But in the Cape Circle the screw readings all diminish as the spring is compressed, so that the reading for greatest wear of the screw requires the greatest + correction; and this corresponds with what we have found above.

As a further test, I mounted four supplementary microscopes. For these Mr. Simms also made new screws in 1880, but they have never been used except in determining the errors of division of the Circle. The screws were therefore practically unworn. Similar observations with those of Table I. for the run of these microscopes were made with the following results, each figure, however, depending on four sets of observations only:

Table II.

Mean Runs of the Screws of the Four Supplementary Microscopes.

Lower Reading of Mic. A.	Equivalent of 5′ of arc.	Excess of Mean.
	r	r
2·0	4·8228	−·0005
3·0	·8230	− 3
4·0	·8228	− 5
5·0	·8247	+ 14
6·0	·8223	− 10
7·0	·8242	+ 11

Here the runs are found to be identical, within limits of error of observation, for all parts of the screw, proving that the whole of the errors exhibited in Table I. have been produced by wear of the screws, and are not due to optical distortion.

As a further test, the following observations of Nadir point were made by Mr. Pett and Mr. Cox in 1884, October 15 :

The ten microscopes were read at each observation, and the Nadir points have been worked out separately for the six microscopes in ordinary use, and for the four microscopes with unworn screws. The results are :

Table III.

Nadir Point.

Reading of Mic. A.	Six Microscopes in daily use.*	Four Microscopes with unworn screws.
r	′ ″	′ ″
5	5 0·99	4 54·55
6	·48	·49
7	·19	·41
8	·08	·42
9	·06	·31
10	·59	·52

The screw corrections (determined for the six microscopes from our discussion of the results of Table I.) we now see more clearly to be defective, because we have forced an expression, which is the equation for a continuous curve, to represent what is in reality a curve terminating at either extremity in a straight line. But if we apply even these corrections to the Nadir points of Table III. which were determined with the six microscopes, we obtain the following more accordant results

* For the results of this column the runs were derived from the usual observations for run.

Reading of Micrometer A.	Nadir from six Microscopes approx. corrected for screw error.*
r	′ ″
5	5 0·62
6	0·57
7	0·73
8	0·78
9	0·52
10	0·47

These much smaller discordances still exceed the errors of observation (as the non-representation of Table I. by the same correction also shows), and I await the arrival of the screw-testing apparatus from England † to make a complete and independent investigation of the errors of each screw.‡

Meanwhile, having shown that, in a period of four and a quarter years, such large errors have been produced in the screws by wear alone, it becomes most interesting and important to determine all that can now be ascertained as to the errors of the screws between 1870 and 1879, when the observations for Mr. Stone's great Catalogue were made. The Transit-Circle was brought into use in 1855, the screws in 1875 had therefore been subjected to wear for twenty years.

The only data that I have been able to find which permit any approximate estimate of the errors of the screws to be made is a series of Nadir observations made in 1877, January 13, 14, 24, and 31. On each of these days the coincidence of the reflected with the direct image of the Declination wire was observed at different readings of the Declination Micrometer screw, whilst the same six divisions of the Circle were read by the Circle Microscopes. If all the screws were free from error, such observations enable us—

1. To find the runs of the Circle Microscope screws if the value of the Declination Micrometer screw is known ; or,

2. To find the value of the Declination Micrometer screw if the runs of the microscope screws are known or determined.

But if the screws of the Circle Microscopes are affected by errors, it is not possible, without a knowledge of these errors, to determine the run, as the run will vary according to the part of the screw employed in its determination. In such a case it is only possible to select some convenient part of the screw as standard to which all our run-determinations shall be referred,

* Here the corrections for screw error were of course applied to the observations before the runs employed were computed.

† The apparatus was sent to Mr. Simms to be adapted to other micrometers used at the Observatory.

‡ It is of course necessary to have some such means to compare smaller portions than 5 revolutions of the screw with each other.

and then afterwards to determine the errors of the screw with reference to this distance.

There exist no data for separating the errors of the Declination Micrometer screw from those of the Micrometer Microscopes; we can therefore only assume, at least in the first place, that the Declination screw is without error, and that, being a steel screw, it is probably much less liable to wear than the gun-metal screws of the Circle Microscopes. The value of the Declination screw was determined by Sir T. Maclear with great care on the following occasions:

	Over arc of 15′.	Over arc of 5′.
1855, Jan. 29	1 rev. = 28″·584†	′
1862, Aug. 22	= ·583*	28·547*
30	= ·584†	·548†

Mr. Stone has, with Sir T. Maclear, adopted the value 28′·548, as the arcs measured with the Declination Micrometer screw are all less than 5′, and I have employed the same value in reducing the Nadir observations of 1877, June 13, 14, 24, and 31.

For the run it was important to determine whether this could be assumed uniform for the dates in question. The unknown errors of the screws rendered it impossible to employ the separate runs determined on each of these dates, because they were made at different parts of the screws, and were not therefore comparable. An examination of the runs, extending over long periods, during which no changes were made in the adjustments of the microscopes, reveals the fact that the apparent variations of run are produced entirely by the errors of the screws, and by the errors of division of the different spaces measured, and that the runs are not sensibly affected by changes of temperature, but that the *real* run is practically constant. The reader can satisfy himself on the latter point by taking the means of the runs in the various months of the year from the Cape Observations for 1876 (when the observers appear to have been instructed to make the lower reading for run between $5^r{\cdot}1$ and $5^r{\cdot}3$), or, better still, from the observations for 1856, when the screws were not worn, and the runs were determined at uniformly distributed parts of the Circle. There was no change in the microscope adjustments in January 1877, and I have therefore re-reduced the Nadir point determinations in question, employing a uniform mean run which represents the run of the screw from the determinations in that month, when the lower reading is about $5^r{\cdot}2$ and the upper therefore about $10^r{\cdot}0$.

* From observations of the Nadir.
† From observations on the South Collimators.

The Nadir readings of 1877, January 13, 14, 24, and 31, were on each day extended over the greater part of the range of the Circle Microscope screws, so that by comparing the Nadirs determined on the different days with similar parts of the microscope screws we can arrive at the real changes of Nadir on the different days, and thus reduce them to a uniform system. In this way the corrections applicable to the results of each day to reduce all to January 14 were found to be:

For Jan.	13	$-0''.15$
	24	-1.03
	31	-1.56

The results so reduced and arranged in order of the readings of Microscope A are the following:

TABLE IV.

Nadir Points observed 1877, *January* 13, 14, 24, and 27.

	Reading of Microscope A.	Observed Nadir Point.
	r	
Jan. 24	4·32	180° 7′ 10″·63
24	·77	10·75
31	·81	10 76
24	·88	10·79
14	·92	10·60
24	5·98	10·42
31	5·03	9·99
24	·08	10·14
31	·19	9·67
24	·21	9·69
24	·33	9·78
14	·34	9·38
31	·37	9·69
24	·42	9·64
31	·54	9·68
24	·62	9 67
14	·76	9·65
31	5·97	9·58
13	6·02	9·28
13	·04	9·31
14	·40	9·49
31	·47	9·29
24	·80	9·31

	Reading of Microscope A.	Observed Nadir Point.
	r	° ′ ″
Jan. 14	·86	180 7 9·53
31	6·97	9·40
13	7·32	9·47
31	·48	9·20
14	·70	9·23
31	·97	9·26
14	7·98	9·47
24	8·06	9·24
13	·26	9·49
14	8·81	9·32
31	9·00	9·62
24	·12	9·43
13	·46	9·37
14	9·84	9·59
31	10·03	9·69
24	·15	9·60
31	·45	10·03
14	·48	10·01
14	·61	10·08
24	·79	9·71
13	·91	10·01

The variations in these Nadir points are the result of the combined errors of the Declination Micrometer screw and of the Circle Micrometer screws, together with the error of the adopted value in arc of the Circle Micrometer screws (i.e. of the adopted run) relative to the adopted value of the declination screw.

For determining the errors of the Circle Micrometer screws it is practically immaterial within certain limits what value of the run is adopted; the effect of changing the value of the adopted run would simply be to produce a coincidence in the value of the Nadir points at two different readings separated by 4·8 revolutions. But as we have employed the mean run derived from independent run-determinations made when the lower reading of Microscope A is near $5^{r}\cdot2$, the Nadir point comes out nearly the same at $5^{r}\cdot2$ and at $10^{r}\cdot0$ (i.e. $5^{r}\cdot2+4^{r}\cdot8$)—a proof that the adopted value of the Declination Micrometer screw is nearly absolutely correct. If therefore the Declination Micrometer screw is free from irregularities, the discordances of the Nadir exhibited in Table IV. are produced by errors which correspond with the mean of the errors of the screws of the Circle Microscopes.

It becomes important therefore to consider what are the probable effects of irregularities in the Declination Micrometer screw on these results. My experience of screws made by Mr. Simms is that their errors are very small when the screws have not been subject to wear, and from the following observations by Sir T. Maclear we may conclude that such was the case in the screw in question.

1862, August 26.

Measurements of the distance between two dots on the cross of the South Collimator, nearly 1r apart, on different parts of the Declination Micrometer Screw.

		Obs.−Mean.			Obs.−Mean.
r	r	r	r	r	r
34·6 to	35·6	−·003	49·5 to	50·5	−·001
35·7	36·7	− 3	50·6	51·6	− 1
36·8	37·8	− 5	51·7	52·7	+ 1
37·9	38·9	− 1	52·8	53·8	− 1
38·9	39·9	− 4	53·8	54·8	− 1
39·9	40·9	+ 2	54·9	55·9	+ 1
41·1	42·1	− 3	55·9	56 9	− 1
42·2	43·2	− 5	57·0	58·0	+ 2
43·2	44·2	− 4	58·0	59·1	+ 3
44·3	45·3	− 1	59·2	60·2	+ 3
45·3	46·3	+ 4	60·2	61·2	− 6
46·3	47·3	+ 7	61·3	62·3	− 1
47·4	48·4	− 3	62·4	63·4	+ 5
48·4	49·5	+ 8			

With regard to these observations Maclear remarks, "The differences from mean do not perhaps exceed the limit of the errors of observation, as the dots observed were not very well defined or regular in outline."

He also measured with the Declination Micrometer two dots nearly 9r·4 apart.

		1st Obs.	2nd Obs.	3rd Obs.	Mean.
r	r	r	r	r	r
35·0 to	44·4	= 9·404	·405	·402	9·404
44·4	53·8	= 9·413	·417	·411	9·414
53·8	63·2	= 9·396	·406	·399	9·399

From an examination of the original observations I find that 50r·0 was adopted as the micrometer reading for the centre of the field, and that the Transit-Circle was clamped so that the star always transited near the centre of the field; thus the vast majority of the observations were made with the reading of the micrometer screw $50^r \pm 1^r$, and that only in exceptional cases were readings of 48r or 52r employed, whilst readings of 47r and

53^r are very rare. From January 1871 till May 1872 it appears to have been the custom to make the Nadir observations when Microscope A read about $0^r{\cdot}3$, corresponding with a declination micrometer reading about $54^r{\cdot}5$. After that date the Nadir readings were made near the middle of the range of the screw—that is, when Micrometer A read nearly $2^r{\cdot}5$ and the Declination Micrometer about $50^r{\cdot}0$. Thus at least 90 per cent. of all the observations were made within one or two revolutions of $50^r{\cdot}0$, and about this point, therefore, we should have the maximum of wear, whilst the screw below 48^r and above 52^r should be practically free from wear. But our Table IV. shows that the *greatest changes in the Nadirs occur* when the readings of Microscope A are below $5^r{\cdot}9$ and above $9^r{\cdot}0$, corresponding with Declination Micrometer readings above $53^r{\cdot}2$ and below $46^r{\cdot}5$—that is, *where the screw is not worn*, and where its irregularities are very small. We are therefore compelled to attribute the whole of the great discordances in the Nadir points shown in Table IV. to errors in the screws of the Circle Microscopes. That is to say, the screws are practically perfect (unworn) from $4^r{\cdot}3$ to $4^r{\cdot}9$, then the errors run down rapidly during the next revolution, are then practically uniform till $9^r{\cdot}5$, when they again run up rapidly, becoming uniform from $10^r{\cdot}4$ onwards. I attribute these errors to wear on the evidence of the first part of the present paper, but Mr. Stone insists so positively that the large errors of the screw which he found below $5^r{\cdot}0$ are due to constraint, that I think it necessary to state more fully my reasons for differing from him on the question.

Mr. Stone puts his argument in the following words:

It may be mentioned that, since the commencement of our re-observation of Lacaille's zones in 1871, negative readings of the Circle Microscopes have been systematically avoided both in the direct observations and in the determinations for the corrections for runs. I noticed that some of the screws of the Circle Microscopes came under constraint when readings below 0^r of Microscope A in the present position of the microscopes and their combs were made. The assistants were, in consequence, instructed not to take readings on the negative side of Microscope A, and such readings have since been but very seldom taken, and then only through inadvertency, and when the readings fell close to the zero. Our Nadir point readings are never taken, for use, with readings of Microscope A so low as 0^r, and are usually taken near the middle of the range of screws.

The following determinations of the reading for the Nadir point may be interesting, as showing the kind of systematic errors which may exist in our work from the combined errors of microscope screws, declination screw, and the assumed values of their revolutions.

1875, *Nov.* 22. *Nadir Point Readings.*

Observer.	Reading of A. 0^r	2^r
E. J. S.	$180^\circ\ 7'\ 26''{\cdot}62$	$26''{\cdot}28$
W. H. F.	$26''{\cdot}55$	$26''{\cdot}37$
G. H. M.	$26''{\cdot}70$	$26''{\cdot}22$
Mean ...	$26''{\cdot}62$	$26''{\cdot}29$

It appears from observations made in 1877, and carried systematically over the whole range of screws, that the difference above indicated is real; but is about as large as the greatest difference obtained throughout the range of the screws employed in the observations.

Errors, which I believe to have the same origin, existed when the screws were new, and are conspicuous in some determinations of the runs made about 1857 and 1858, when occasionally the starting-point for the run was taken some distance on the negative side of the zero. Such errors may, unless carefully guarded against, become of serious amount. The source of these errors may, in my opinion, be indicated as follows:

The effective range of the screws in the Cape microscopes does not extend, at most, over more than six revolutions; for the last two threads certainly, and probably the last three near the base of the screw, are not so highly finished as those threads near the middle range of the screw which, I suppose, were intended to be, and which at all events have been, most generally used.

It requires, therefore, some care to adjust the microscopes so that the several readings shall not differ greatly among themselves, and yet all the screws work sweetly over the run of a 5′ division on the circle. In all cases when the last two or three threads become engaged the commencement of constraint can be felt and the readings at once begin to become defective. When the runs are determined from a starting point at which some of the microscopes are under constraint, while the readings at the other end are taken with microscopes free from constraint, the result may be that the error arising from constraint will be carried by the run correction to its full extent, and with the same sign into all readings made near the other end of the run; at intermediate points the errors will be smaller and will decrease with great rapidity until the state of constraint be passed, after which they will only depend on the erroneous correction for runs, and will increase proportionally to the reading. It has been to escape the use of any of the microscopes under constraint that negative readings have been avoided.

With reference to these remarks, it is well to examine the evidence as to the runs with negative readings in 1855 when the screws were new (i.e. the runs when the lower reading is below $5^{r}{\cdot}0$).

There are only two such readings recorded in 1855, but I give also the two first of the same kind in 1856. Fortunately there are also other determinations of run on the same days for comparison.

Determinations of Run in 1855 *and* 1856 *at different parts of the Circle Microscope Screws.*

	Lower Reading of Circle Microscopes.	Pointer Reading.	Run.
	r	°	r
1855, Nov. 25	4·76	70	4·804
	5·46	90	·820
Dec. 5	4·74	360	4 812
	5·24	240	·804
1856, Jan. 6	4·53	270	4·813
	5·64	250	·813
Feb. 17	4·90	60	4 818
	0·36	80	·814

The mean result is that:

For negative readings the run = 4·812
For normal „ = 4·813

There is nothing here to justify the remark that errors having origin in constraint were "conspicuous" when the screws were new.

Let us now take some similar examples of run at a much later period. There are no examples of runs with negative readings taken after December 1875. The following are the whole of the examples that occur in 1874 and 1875:

	Lower Reading of Circle Microscopes.	Pointer Reading.	Run.
	r	o ′	r
1874, June 7	4·7	34 55	4·778
	5·3	31 20	·791
July 21	4·7	35 35	4·783
	5·7	36 10	·795
Nov. 28	4·8	36 10	4·783
	0·3	37 35	·798
1875, July 6	4·8	26 30	4·784
	5·7	27 15	·797
Oct. 28	4·8	313 25	4·783
	5·5	25 40	·803

In the mean:

When negative readings are employed the run is = r 4·782
When normal „ „ = ·797

What is the cause of this difference—a difference that did not exist when the screws were new, but which is so marked after eighteen or twenty years' wear? Mr. Stone says it is "constraint" produced because the last two or three threads near the base of the screw are not so highly finished as those threads near the middle of the range. But this theory is incompatible with the following facts:

1. A careful examination of one of the old screws (the screw of an original spare microscope micrometer that can be adapted to the pointer for special purposes) shows that there is no difference in finish between the threads near the base and the rest of the screw. There is, in fact, no shoulder to the screw, but the thread has been cut for several revolutions beyond its present terminal point, and then these threads have been removed by turning. Anyone conversant with the process of making such a screw also knows that it would be much more troublesome and difficult to finish one part of the screw less

carefully than another part than to finish all parts alike, and that Mr. Simms is not likely to take such trouble in order to make the screws designedly imperfect.

2. If there is such constraint as Mr. Stone describes, it is quite inexplicable why it should not exist in 1855–56, and be so very marked in 1874 and 1875.

3. Such constraint is utterly inconsistent with the facts of Table IV., which show that from $4^{r}\cdot3$ to $4^{r}\cdot9$ the screws are perfectly uniform.

Mr. Stone appears to have somewhat incautiously ascribed the errors to constraint because sometimes the action of the screws became stiff for readings below $5^{r}\cdot0$. But if the screws are very seldom used below $5^{r}\cdot0$, there is no doubt that immediately below $5^{r}\cdot0$ there would be a considerable and abrupt accumulation of dirt and congealed oil which would render the screws difficult of rotation for readings below $5^{r}\cdot0$; or there might have been a mal-adjustment of the micrometer box relative to the screw, as I have shown to be possible before the bearing of the shoulder of the screw was attached to the box. One cannot now precisely say what caused the "constraint" mentioned by Mr. Stone; I can only state that there are occasional notes, such as "the screws turned stiffly," followed soon after by a note that the screws were cleaned, and then there are no more notes about stiffness of the screws for a long time.

But if "constraint" did exist, one is puzzled to understand how it can explain the results of Table IV. Does Mr. Stone mean that the difficulty of moving the slide was such as to create a tension on the screw sufficient to elongate it $\frac{1}{50}$ of a revolution (or to twist its head relative to the thread engaged by $\frac{1}{50}$ of a revolution), or to bend the thick and solid projection which carried the hollow bearing of the shoulder of the screw by this amount, and that the materials were so perfectly elastic as to perfectly recover their original form and exactly reproduce the same amount of error due to constraint on each occasion? One feels sure that Mr. Stone could not mean this, and yet how otherwise could constraint affect the readings?

But if further proof is required of the erroneousness of Mr. Stone's conclusions, we may examine the errors of the runs at various epochs, and if these errors change progressively we may be satisfied that they are produced by wear.

This is sufficiently exemplified by the following results. The figures inclosed in brackets denote the number of observations upon which each result depends:

Lower Reading of Microscope A.	1856–57.	1872–73.
r r	r	r
4·5 to 4·6	4·811 (3)	
4·6 4·7	·812 (9)	
4·7 4·8	·811 (11)	
4·8 4·9	·811 (19)	4·808 (1)

Lower Reading of Microscope A.		1856–57.	1872–73.
r	r	r	r
4·9 to	5·0	4·811 (26)	
5·0	5·1	·811 (19)	4·821 (11)
5·1	5·2	·813 (28)	·823 (21)
5·2	5·3	·814 (28)	·826 (29)
5·3	5·4	·814 (19)	·828 (40)
5·4	5·5	·815 (15)	·830 (39)
5·5	5·6	·816 (7)	·831 (35)
5·6	5·7	·815 (7)	·831 (15)
5·7	5·8		·833 (6)
5·8	5·9		·835 (4)
5·9	6·0		·831 (6)

After 1873 there is no considerable period without some change in the microscope adjustments, and in which there is any variety in the part of the screw employed in the run determination. Thus, for example, in 1878–79 the lowest reading for run varies only between $5^{r}\cdot3$ and $5^{r}\cdot6$. A false impression as to their real uniformity is thus created. There are here and there readings for runs below $5^{r}\cdot0$, which are sometimes followed by readjustments of the microscopes, and notes by Mr. Stone that such runs are wrong, but there is no step taken to determine the cause of the errors or to permit anyone else to do so.

Let us now consider the effect of wear upon a screw. In so doing it will be convenient to imagine the screw unrolled and represented by a wedge, of which A B is the surface that bears upon the female screw cut in the slide, and C B the bearing shoulder.

FIG. 1

Then if the shoulder is true, C B will be a straight line, and if the threads are equal and free from "drunkenness" A B will also be a straight line. I find on examination that the bearing or female screw embraces eight threads of the micrometer screw; we may therefore represent its bearing surface by that of a block D E extending over eight revolutions of the screw. If this block

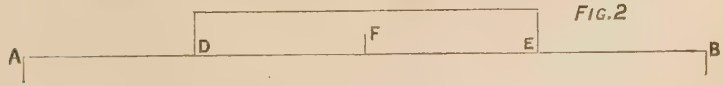


FIG. 2

D E is rubbed continually on A B, so that its middle point F

(fig. 2) is brought alternately as far as A and B, then if the surfaces are each uniform in texture throughout, the surfaces, however long the abrasion is continued, will remain plane.

If D E has a shorter sweep, so long as E comes as far as B, and D as far as A, there will be a tendency to make the surface A B slightly hollow and D E slightly convex.

But if the sweep of D E is limited to the points G H, and if we suppose for the moment that the surface D E is incapable of wear, then A B must become worn in the form (fig. 3).

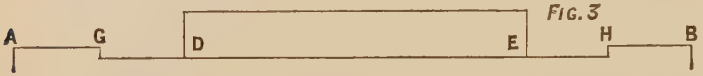


FIG. 3

If we suppose further that the wear, instead of being produced by sweeps which always extend from G to H, is produced by the ordinary use of a micrometer screw—that is to say, if the successive sweeps begin as often at $5+n$ revolutions as at $5+2n$ revolutions, as at $5+3n$ revolutions, and so on, but limited to $5+m$ revolutions, and as a necessity must end as often at the point $5+n$ as at $5+2n$, as at $5+3n$, &c., also limited to $5+m$ revolutions, then the wear of A B will be represented by G *d e* H (fig. 4), where *d e* is a straight line equal in length to D E, and G *d* and A *e* are also straight lines represented in fig. 4.

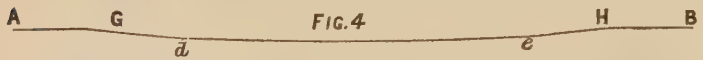


FIG. 4

But these supposed cases cannot occur in practice, because the bearings D E must wear. The calculation of the form of D E and of the now continuous curve G *d e* H therefore involves a knowledge of the relative coefficients of wear of the screw and its bearing, and is further complicated, when one endeavours to go to the root of the matter, by the form of the section of the thread of the micrometer screw and the fit of the female screw. The general form of the wear is, however, for the two surfaces nearly as shown in fig. 5.

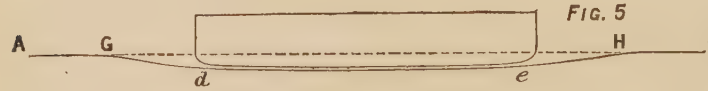


FIG. 5

To obtain in a very easy manner the general form of the resulting errors in the screw readings, imagine the following simple construction:

Let *a s t b* (fig. 6) be a piece of thin wood, of which the upper edge *a g h b* is cut out to a curve representing the worn thread of the screw, and the lower edge *s t* is a straight line parallel to the straight parts of *a b*. On the upper edge of *a b* let there rest *d e*, the lower edge of which is shaped to represent the worn surface of the female screw. Attach a wire *p w* rigidly to *d e*

at right angles to the original unworn surface, and suppose this wire to move smoothly through holes in the fixed blocks r and v.

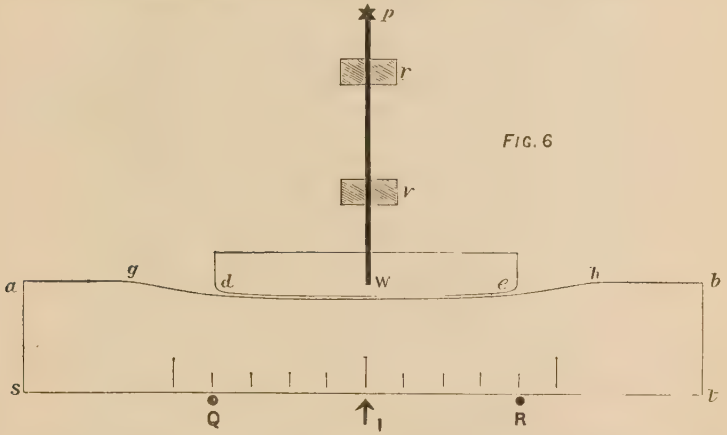


FIG. 6

Let Q and R represent points (immoveable with respect to r and v), and let $s\,t$ always rest on these points, but be moveable from right to left, or *vice versa*, upon them. Further, let there be an index at I and a graduation on $s\,t$ representing the corresponding readings of the screw (where the reading at I must correspond with the middle reading of the range of the screw when $d\,e$ is in the middle of its range). Since the wire $p\,w$ sliding in the fixed blocks r and v preserves the parallelism of the original (unworn) surface of $d\,e$ with $s\,t$ (that is, represents the preservation of the parallelism of the axis of the male and female screw which in the micrometer is accomplished by the slide), then, whatever point of the curve $g\,h$ comes in contact with $d\,e$, that point represents the part of the worn surface of the screw which will be engaged for the corresponding reading of Index I, and the rise and fall of the point p (the end of the wire), as $s\,t$ is moved on the points Q and R, will represent the variations of the errors of the screw, corresponding to the various readings of the Index I.

It is obvious, from mere inspection of fig. 6, that for the central parts of the screw the errors will vary very slowly, but that near the extremities of the range they will change with very great rapidity, as we have seen to be the case in practice—viz. in Table IV.

Mr. Christie, in his paper "On the Effect of Wear on the Micrometer Screws of the Greenwich Transit-Circle" (*Monthly Notices*, vol. xxxvii. p. 20), ascribes the large errors between 0^r and 1^r (i.e. in our notation between 5^r and 6^r) as due to a preponderance of a large number of readings which have produced additional wear near that point.

Mr. Stone (*Monthly Notices*, vol. xlii. pp. 33 and 34), in his

note on these conclusions of Mr. Christie (repeated in a series of papers by him on the Greenwich refractions), expresses the following opinion:

> Neither 4.000 nor 8,000 additional Nadir observations made near 0r·0 in a period of eight years would have led to any such serious wear of the screws as that indicated as existing in 1876 near 0r·0. If all the observations were made near 0r·0, and the screws were never worked far from this reading, such a thing might be conceivable, but in practice the screws are worked continually backwards and forwards over the whole range of the screws from 0r to 5r.

Mr. Stone then proceeds to give "a key to the true explanation of these errors" by the extraordinary statement:

> In all screws the threads near the end of the screws are defective, and are never intended to be used.

Now it is well known that Mr. Simms has an excellent screw-cutting engine, and a very limited knowledge of the use of such an engine would show that it would be much more difficult for him to make the threads beyond the normal range defective than to make them as perfect as the rest. Why, therefore, should one assume that in every case trouble is taken to make the last threads of the screw defective? In one case, that of the unworn screws of the Cape Transit-Circle Microscopes α, β, γ, and δ, it is shown in Table II. that they are practically perfect for a much greater range than that in common use, viz. from 2r·0 to 11r·8. I have also shown from abstract considerations that if the screws are worked continually backwards and forwards, as in practice, errors resulting from uniform wear will produce errors in the readings which increase very rapidly towards the limit of the range.

I have also shown from Mr. Stone's own observations of Nadir points in January 1876 that errors from wear, similar with those of the Greenwich screws, existed in the screws of the Cape Transit-Circle.

It remains for me now to offer an explanation of the only remaining point, viz., why the errors near 5r are greater (or rather, change more rapidly) than those near 10r. It appears to be simply this:

The wear of 5r must always be greater than at 10r because the spiral springs are much further compressed (and therefore much greater friction is created) at the former than at the latter reading. Further, if the male screw is so short that beyond 10r the female screw extends over it, the form of the curve of wear will obviously be much flatter, but I have no means of deciding now whether both causes or only the former cause operated in the case of the old screws.

In the accompanying figure (fig. 7) the separate results of Mr. Stone's Nadir observations, 1876, are laid down, and a curve

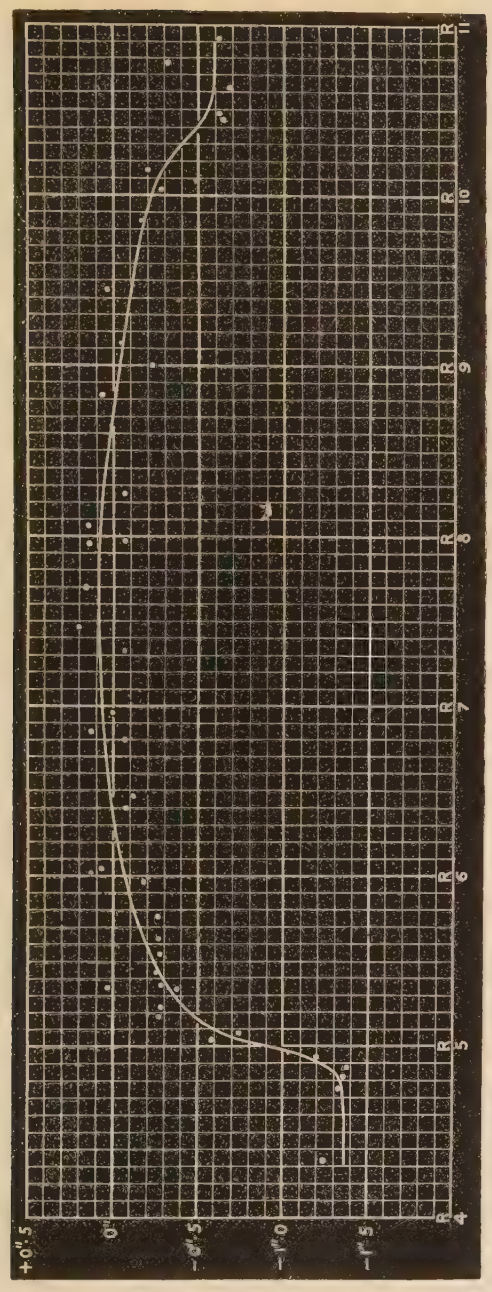


FIG. 7.—Curve showing corrections to Nadir Point according to readings of circle microscopes, deduced from observations made by Mr. Stone in January 1877.

has been drawn to represent the observations as accurately as possible.

From this curve the following Table of corrections is obtained:

TABLE V.

Corrections to be applied to the readings of the Micrometer Screws of the Cape Transit-Circle, deduced from Nadir observations made in January 1877.

Rev.	"	Rev.	"	Rev.	"	Rev.	"
4·3	−1·35	6·0	−0·07	7·7	+0·10	9·4	−0·10
·4	−1·35	·1	− ·05	·8	+ ·10	·5	− ·11
·5	−1·35	·2	− ·03	·9	+ ·10	·6	− ·13
·6	−1·35	·3	− ·01	8·0	+ ·09	·7	− ·15
·7	−1·35	·4	·00	·1	+ ·08	·8	− ·17
·8	−1·35	·5	+ ·01	·2	+ ·07	·9	− ·20
·9	−1·20	·6	+ ·02	·3	+ ·06	10·0	− ·23
5·0	−0·95	·7	+ ·03	·4	+ ·05	·1	− ·27
·1	−0·60	·8	+ ·04	·5	+ ·04	·2	− ·32
·2	−0·47	·9	+ ·05	·6	+ ·02	·3	− ·40
·3	−0·36	7·0	+ ·06	·7	·00	·4	− ·50
·4	−0·29	·1	+ ·07	·8	− ·02	·5	− ·56
·5	−0·24	·2	+ ·08	·9	− ·04	·6	− ·58
·6	−0·20	·3	+ ·08	9·0	− ·05	·7	− ·59
·7	−0·15	·4	+ ·08	·1	− ·06	·8	− ·60
·8	−0·12	·5	+ ·09	·2	− ·07	·9	− ·60
·9	−0·10	·6	+ ·09	·3	− ·08	11·0	− ·60

Remembering that 5′ of arc corresponds nearly with 4·8 revolutions, we obtain from Table V. the following Table VI. for correcting the runs on account of errors of the screws.

TABLE VI.

Corrections to be applied (in arc) to the observed equivalent of 5′.

Argument. Lower Reading of Microscope A in Observing Run.	Correction to Equivalent of 5′.	Argument. Lower Reading of Microscope A in Observing Run.	Correction to Equivalent of 5′.
Rev.	"	Rev.	"
4·7	+1·24	5·5	−0·16
4·8	+1·22	5·6	− ·30
4·9	+1·15	5·7	− ·41
5·0	+0·78	5·8	− ·46
5·1	+0·40	5·9	− ·49
5·2	+0·24	6·0	− ·53
5·3	+0·09	6·1	− ·55
5·4	−0·03	6·2	− ·56

The complete correction of an observed N.P.D. for errors of the screws can then be easily obtained from these Tables as follows:—

Put R = the lower reading of Microscope A in the observed determination of run.

N = the reading of Microscope A in the determination of Nadir.

O = „ „ observation of the star.

r = the correction from Table VI. taken out with the argument R.

n = „ Table V. taken out with the argument N.

o = „ Table V. taken out with the argument O.

Then the correction to be applied to an observation of N.P.D. (if the adopted latitude is correct) will be (since the readings of the circle increase as the telescope is turned from the Zenith to the South):

$$r\left(\frac{\mathrm{N}-\mathrm{O}}{4^{\cdot}8}\right)+o-n.$$

Unfortunately these corrections cannot be embodied in a single Table, but in order that the reader may judge of their influence on the results, two Tables have been prepared, viz.:

Table VII., showing the resulting corrections when the Nadir has been observed at $5^{r}\cdot3$, and

Table VIII., showing the corrections when the Nadir has been observed at $7^{r}\cdot5$.

From January 1871 till May 1872 the Nadir point observations at the Cape were made with the readings of Microscope A about $5^{r}\cdot3$. This was the practice introduced by Mr. Stone at Greenwich in 1868, a practice which, on mistaken grounds, he defends in his "Note on Mr. Christie's Paper" (*Monthly Notices*, xlii. p. 32). Mr. Stone's defence, indeed, is the more strange, seeing that he defends a practice in 1881 which he himself had relinquished in 1872. After May 1872 the Nadir is observed more nearly towards the middle of the screws (i.e. $7^{r}\cdot5$).

A comparison between Tables VII. and VIII. will show the large and sudden change which may be introduced into the system of N.P.D. results, viz. of about $+0''\cdot5$—a change, however, which would have been for the better if the latitude depended upon the same observations.

In the Introduction to the Cape observations, previously quoted at length, Mr. Stone states that a difference of $0''\cdot33$ between two observations of Nadir, which he quotes, is real, but *is about as large as the greatest difference obtained throughout the range of the screws*; and describes such a difference as "*the kind of systematic errors which may exist in our work from the combined errors of microscopic screws, Declination screw, and the*

TABLE VII.

Nadir supposed taken at 5ʳ·3.

Lower Reading of Microscope A in determining run.	Reading of Microscope A in observing object.											
	4·3	4·9	5·0	5·1	5·3	6·0	7·0	8·0	9·0	10·0	10·5	11·0
4·8	0·86	−0·74	0·51	−0·19	0·00	−0·11	−0·01	−0·24	−0·63	−1·07	−1·52	−1·69
4·9	0·87	0·75	0·52	0·19	0·00	0·12	+0·01	−0·20	0·58	1·00	1·45	1·61
5·0	0·91	0·78	0·54	0·21	0·00	0·17	+0·14	+0·01	−0·29	0·64	1·05	1·17
5·1	0·94	0·80	0·56	0·22	0·00	0·24	0·28	+0·23	0·00	0·26	0·63	0·71
5·2	0·96	0·82	0·57	0·23	0·00	0·26	0·33	0·32	+0·12	−0·11	0·46	0·52
5·3	0·98	0·83	0·58	0·23	0·00	0·28	0·39	0·40	0·24	+0·04	0·30	0·35
5·4	1·00	0·85	0·60	0·25	0·00	0·29	0·43	0·46	0·33	0·16	0·17	0·21
5·5	1·01	0·86	0·60	0·25	0·00	0·31	0·47	0·54	0·43	0·28	0·03	0·05
5·6	1·02	0·86	0·61	0·25	0·00	0·34	0·53	0·62	0·54	0·43	0·13	0·12
5·7	1·03	0·87	0·61	0·26	0·00	0·35	0·57	0·68	0·63	0·53	0·25	0·25
5·8	1·04	0·88	0·62	0·26	0·00	0·36	0·58	0·71	0·66	0·58	0·30	0·31
5·9	−1·05	−0·89	−0·63	−0·27	0·00	+0·36	+0·59	+0·72	+0·68	+0·61	−0·33	−0·34

Table VIII.

Nadir supposed taken at 7ʰ·5.

Lower Reading of Microscope A in determining run.	Reading of Microscope A in observing object.											
	4·8	4·9	5·0	5·1	5·3	6·0	7·0	8·0	9·0	10·0	10·5	11·0
4·8	−0·75	−0·63	−0·40	−0·08	+0·11	+0 22	+0·10	−0·13	−0·52	−0·96	−1·41	−1·58
4·9	0·79	0·67	0·44	0 11	+0·08	0·20	0·09	0·12	0 50	0·92	1·37	1·53
5·0	1·00	0·87	0 63	0·30	−0·09	+0·08	0·05	0·08	0·38	0·73	1·14	1·26
5·1	1 21	1·07	0·83	0·49	0·27	−0·03	+0 01	0·04	0·27	0·53	0·90	0·98
5·2	1·30	1·16	0 91	0·57	0·34	0·08	−0 01	0·02	0 22	0·45	0·80	0·86
5·3	1·39	1·24	0·99	0·64	0 41	0·13	0·02	−0 01	0·17	0·37	0·71	0·76
5·4	1·46	1·31	1·05	0·71	0·46	0·17	0·03	0·00	0·13	0·30	0 63	0·67
5·5	1·53	1·38	1·12	0·77	0·52	0·21	0·05	+0·02	0·09	0·24	0·55	0·57
5·6	1·61	1·45	1·20	0·84	0·59	0·25	0·06	0·03	0·05	0·16	0 46	0·47
5·7	1·67	1·51	1·25	0·90	0 64	0·29	0·07	0·04	−0 01	0·11	0·39	0·39
5·8	1·70	1·54	1·28	0·92	0 66	0·30	0 08	0 05	0 00	0 08	0 36	0·35
5·9	−1·72	−1·56	−1·30	−0·94	−0·67	−0·31	−0·08	+0·05	+0·01	−0·06	−0·34	−0·33

assumed values of their revolutions." It would be interesting to know how Mr. Stone reconciles the former of these statements with the facts of Table IV. observed by himself, and how he reconciles the latter statement with the facts of Tables VII. and VIII.

The general conclusions appear to be:

1. The errors of screws of Transit Circles require much more frequent examination than it has been customary to give them.

2. Steel screws should be substituted for the present gun-metal screws of the Cape Circle Microscopes, the latter being far too subject to change from wear.

3. Three of the six microscopes should be reversed (as has already been done at Greenwich), so that the springs may press the bearings of the screws in the opposite direction. This precaution, however, is not a complete cure, as the errors produced by wear affect the screw readings most near the point of greatest compression of the spiral springs, and the curve of correction is thus not symmetrical with the middle of the range. Besides this, it will be seen from Table I. that the wear may be very different for the six screws, although doubtless they were supposed to be made of precisely the same material.

4. The N.P.D. results of the *Cape Catalogue* for 1880 must be corrected for screw errors, and the latitude determination be rediscussed before these results can be admitted into any fundamental determination such as that of the constant of refraction. It is not improbable that the large *cosine* term in the flexure which is so marked in 1871 and 1872 may be due in part or in whole to errors of the screw, coupled with systematic observation of the Nadir near $5^{r}\cdot3$, as the N.P.D. of each star will be affected by errors which have a considerable + sign for the most usual readings (see Table VII.).

Royal Observatory,
Cape of Good Hope
1884, *Nov.* 5.

Spectroscopic Observations made at the Earl of Crawford's Observatory, Dun Echt, Aberdeen. By Ralph Copeland, Ph.D.

Under the direction of Lord Crawford a number of sweeps over the heavens have been made here this autumn in search of small nebulæ and other objects with remarkable spectra. The instrument used is the 6·06-in. Simms Equatorial, with a Secchi prism in front of the object-glass. In examining the richer part of the milky way about *Cygnus* four nebulæ and one star with a spectrum of bright lines, which seem not to have been pre-

viously noticed, were found in the earlier sweeps. Later sweeps in the same part of the heavens have not added to the list, although many known nebulæ and red or variable stars have arrested attention. On September 22 Comet Wolf was found by its spectrum, twelve hours before the news of its discovery had reached Dun Echt, the announcement having been accidentally delayed in the telegraph office at Kiel.

The places of the five objects are :—

R.A. 1884.	Decl. 1884.	When found.	Remarks.
h m s	° ′ ″	1884.	
20 5 9·82	+16 34 53·3	Sept. 17	Equal in brightness to a star 10·2 mag. Diam. *n* and *s* 4″·6 by micrometer. It has an 11 mag. star at 222° 27′, distant 84″·2.
20 6 44·09	+46 7 2·3	Sept. 20	Planetary nebula; most of the light in a single line.
20 7 33·43	+19 38 30·4	Sept. 17	Nebula about 2½″ diam., equal to a 9·8 mag. star. Follows *η Sagittæ* $7^m\ 32^s{\cdot}77$, 61″·15 to the south.
20 7 52·99	+38 0 26·8	Sept. 22.	This is the 7·1 mag. star, D.M. +37°,3821. It has a spectrum of several bright lines near D, and a very bright band in wave-length 464 ^{mmm}. The place is from B.W.2.
20 9 40	+12 23 3	Sept. 22	This seems to be identical with the 9·5 mag. star D.M. +12°,4266. It is in reality a planetary nebula about 4″ in diameter with a nearly monochromatic spectrum.

The fourth object is probably the most interesting of the series. It is practically a member of the same group as the three stars of the same type found by Messrs. Wolf and Rayet to which Professor Pickering has added a fourth. This is the brightest object of its class in the northern heavens. It is intended to examine its spectrum shortly with improved appliances.

Dun Echt:
1884, *Dec.* 11.

An Occulting Eye-piece. By Edmund J. Spitta, M.R.C.S. Eng., L.R.C.P.Lond.

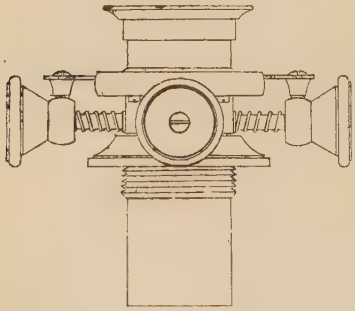
This arrangement consisting essentially of a shallow cylindrical brass box containing four shutters, placed in front of a tube carrying the lenses, is convenient in construction, and when screwed in to the telescope in place of an ordinary eye-piece, does not interfere with the comfort of the observer, is easily under his command, and not weighty enough to require a counterpoise.

The box containing the shutters, about a quarter of an inch deep, and one and a half in diameter, is pierced at its circum-

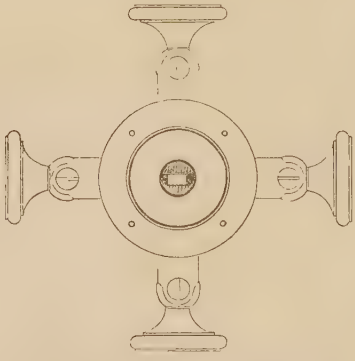
ference by four dove-tailed grooves, planed to allow the "carriers" of the shutters which run in them to move smoothly and regularly to and fro. Attached to the outer end of each carrier, at right angles, is a gun-metal pillar, perforated to hold the milled-headed screw—60 threads to the inch—the observer uses to close or open the shutters, whilst at its central end, that really within the confines of the box itself, is attached the shutter, its occulting end being "knife-edged." Great difficulty was experienced at first in making the four shutters meet in the field without catching against one another, and yet to keep all four in focus. At length this was overcome by bending two opposite ones deeply, the occulting edges being only in the focus, and allowing the others to pass to and fro within the curves of the former two. All are now in focus, and there is no fear of collision.

If one of the screws be turned in the ordinary direction, the corresponding shutter is pushed forward, and the field in that part is gradually occulted, whereas if all four be employed it will be reduced to a four-sided, right-angled figure. By further manipulation it can be shaped into a slit like that of the spectroscope, or minimised still more until it becomes a mere pin-hole.

The lenses, Ramsden's construction, are placed in the cylinder, fixed to the shutter-box, and each is adjusted for its own focus of the shutters—powers 120 and 200 on a 6-foot. Three caps to this cylinder are provided, one with a small and another with a large eye-hole for small or large fields, and a third fitted with tinted glass which screws on the other two for viewing the Sun.



Side View with Sun Cap.



Viewed from above. Lenses and Cap removed showing Occulting Shutters.

The uses for which this instrument may advantageously be employed are as follows: Occulting bright stars whilst examining the fainter ones around;* the planets whilst viewing their

* In using this instrument the observer must bear in mind that it takes an

satellites, especially those of *Jupiter* in transit; portions of the Moon, the details thus shown being sometimes very striking; Sun spots or their nuclei, no diagonal being needed if the observer screws up the shutters very close before looking through the telescope; searching for comets or examining nebulæ and clusters.*† As no clock is required for this instrument, and as all the shutters are at right angles to their neighbours, position angles can be very shrewdly calculated. To do this let the major star run along the edge of one or other shutter by turning the whole instrument on its axis, and bring a neighbouring shutter into the field. As the ordinary circular field is now reduced to an arc of 90° the Pos. Angle can much easier be guessed.

Due thanks must be accorded to Mr. Mason, the optician, who resides in my neighbourhood, for his great ingenuity and patience in perfecting many manufacturing details in this contrivance, and to Mr. Wray, jun., who especially designed the deep positive eye-pieces suitable to my requirements.

The Long Duration of Meteoric Radiant Points.‡

By W. F. Denning.

I have several times taken occasion to mention the very extended duration of a considerable number of the diverging centres of shooting stars, and the fact that there are successive recurrences of meteors from exactly the same points of the sky after short intervals of apparent quiescence. There are, however, grave mathematical difficulties to the view that such streams are physically associated. The fact of stationary radiants exhibiting visible activity during several months is a phenomenon so unaccountable and so utterly opposed to the approved theories as to the orbits of shooting stars, that it must receive a most crucial examination before it can be accepted. And the only applicable

appreciable time for the eye to recover the effect of the previous glare of the bright star; for that purpose it is well, I find, to keep the bright object some while behind a shutter before attempting to dismiss the field as barren. After a steady gaze a small star near *a Sagittæ* was observed Sept. 12 distinctly with the occulter, but *not visible* with my aperture (10 inches) whilst employing ordinary means. We judged its P.A. 180. Mr. Burnham wrote to the *Eng. Mech.* some weeks after to say he had discovered it with the great instrument he uses. He judged P.A. 180·5 with his micrometer.

* By loosening a small screw in the corresponding milled head of two of the shutters, they can be removed at once, and a blot or specially designed device easily substituted. This, it has been suggested, would be required for celestial photography in using the double printing process in conjunction with the occulter.

† It is worth a passing note that in using the occulter on clusters, the colours of particular stars are *much* better observed when neighbouring luminaries are absent. The contrast with and without the use of the shutter is very striking.

‡ For references to this question see also *Monthly Notices*, vol. xxxviii., pp. 111, 115, and 351.

and safe test is by observation prolonged to the utmost limits, and made as accurate as the conditions will possibly allow.

It must be conceded that a well-attested fact of observation, however hard to reconcile with known theories, ought on no account to be disregarded because of its nonconformity. Indeed, such facts should invariably be published, so that the evidence may be sifted and, if possible, the sources of difficulty removed. In a very modern and imperfectly developed branch like meteoric astronomy it is all the more necessary to mention discordances which may exercise an important bearing upon the theory of the subject. In this connection we might well remember the words of Humboldt,* which, though written some years ago, seem equally applicable to the present time:—"The progress of our knowledge respecting igneous meteors will be the more rapid the more impartially facts are separated from opinions, so that while carefully sifting or testing all particular facts on the one hand, we may not, on the other, fall into the error of rejecting as bad or uncertain observations whatever results we are not yet able to explain. It appears to me most important to separate physical relations from those geometrical and numerical relations which admit, generally speaking, of more certain and assured investigation."

It has been attempted to explain the evidence of long enduring meteor showers on the supposition of many successive though independent streams intersecting the Earth's orbit and displaying nearlythe same astronomical radiant points. The immense concourse of meteoric systems annually visible, the difficulties attending their precise observation, and the liability to obtain pseudo-radiants by the accidental convergence of meteors not physically connected, have been mentioned as the probable origin of the observed anomalies. No one more freely than myself will admit the seemingly infinite number of meteor streams, and the practical difficulties in effecting their disassociation, and in accurately placing their radiant centres. But with experience these obstacles become less formidable; the observer gains confidence with greater aptitude, and is led by many circumstances to rely upon his work. He finds it expedient to connect the shower meteors by their individual features of resemblance. The apparent lengths of the paths, occurrence of streaks or trains, foreshortened tracks, &c., are to him details of the greatest significance in attributing the true radiant points, and cannot possibly be disregarded in reliable investigations of this kind. Observers soon become familiar with such appearances, and will not fail to recognise that, combined with the direction of motion (which in itself is often an insufficient guide), they afford the essential means of securing results of the most trustworthy and accurate character.

Consecutive meteoric displays having no natural connection, though issuing from the same region, must necessarily exhibit

* *Physical Description of the Heavens*, translated by Col. E. Sabine, 1866, p. 420.

small differences of position. They will disagree to some 5°, 7°, or more, and it will become evident from such palpable displacements that they are quite independent systems, unless indeed, similarly to the August *Perseids*, the radiant shifts amongst the fixed stars. But my observations absolutely prove that during several months the position of the radiant is unaltered ; it adheres tenaciously to a stationary position. There are very slight differences of 1° or 2° between some of the determinations, but these are obviously due to the errors of observation which cannot be wholly eliminated from such determinations, though they are reduced to a minimum by great accuracy in recording individual meteors. In cases where the radiant adjoins a conspicuous star it is remarkable that wherever the point of divergence is first seen relatively to the star, so it remains during the whole interval of its sustenance. This could not possibly be the effect were the displays a mere chance grouping of streams perfectly disassociated. I regard this peculiarity as a most significant one, and feel assured that it will stand the test of more extended and refined observation. Indeed, the more accurate and full the further investigation may be, the most concisely and indisputably will this fact of long duration become manifested.

It has been said that even if a radiant point were found persistent for several months by duplicate observations of meteors and other corroborative means, it could not be called one stream, because the particles must pursue different orbits. It would consist of a series of streams arranged in a peculiar manner, but not physically associated in a common orbit. This may or may not be the correct interpretation of the fact. In any case, it is obvious that multiple observations either of isolated fireballs or ordinary shooting stars will never influence the question at issue to any material extent, for such observations are usually accidental, and altogether too wild to affect an investigation in which great precision is of paramount importance. It must be admitted that it is a usual thing for the radiant points of fireballs, derived from a discussion of several observations, to show probable errors of 5°, 7°, or even 10°, whereas an isolated observer who closely watches the progress of a star shower may get its centre certainly within 2°, and very often within 1°. Even in cases where few meteors are noticed this is very practicable, especially should the radiant be low, and the meteors belong to the streak-leaving class. Occasionally, no doubt, a combination of the recorded flights of multiple-observed meteors results in accurate positions (the great fireball from *Taurus* on November 23, 1877,* may be referred to as an example), but it is an exception to meet with observations of the required precision.

* See *Observatory* for Jan., Feb., and March 1878, and *Astronomische Nachrichten*, No. 2566, where Von Niessl's computations of several fireballs from *Scorpio* (248°·3 − 20°·1) in June and July exhibit a remarkably close agreement.

It may also be mentioned that stationary meteors, though giving undeniable evidence of radiants, cannot be accepted as affording the precise indication of their positions. These so-called stationary meteors, registered in various catalogues, are usually much foreshortened tracks (perhaps $\frac{1}{4}°$ or $\frac{1}{2}°$ in length), which are very difficult to observe as regards exact direction, owing (especially in the instance of pretty bright meteors) to the dense offcome of sparks from the nucleus, or to the phosphorescence it generates as the result of concussion with the air. Luminosities of this appearance lying in the immediate wake of meteors will give an ill-defined, uncertain character to the slight motion that might otherwise be sharply perceptible. Moreover, such meteors often show a sinuous or curling motion, which is imparted to them in the act of penetrating atmospheric layers of different densities; * and this effect must obviously increase the difficulty in recording them with any approach to exactness. I have compared the positions of many alleged stationary meteors with the sharply defined radiant points to which they severally belonged, and the discordances are often as much as 5°.

Since the evidence of unusual permanency in radiant points first impressed itself on my notice I have given special attention to the circumstance, as it seemed a very important one, involving some mystery not yet explained in relation to these phenomena. It has been my aim, therefore, to register meteor paths with extreme care, and to reject all observations of doubtful character. I experienced a difficulty at first in retaining the *directions* with sufficient certainty while noting the *positions* of the beginning and end points relatively to adjacent stars. Doubt will often arise in the few seconds after the first impression, and while the observer is noting the place of appearance for the purpose of transferring the path to the chart, some means of aiding the memory in the exact retention of the directions seemed requisite. I found it of great assistance to project a perfectly straight wand, held at arm's length upon the track of each meteor as it appeared, then the eye readily assigned the fundamental points, and the path was accurately reproduced on the map. In cases where the meteor left a streak, or where the visible trajectory was slow, this proved a most effective means of obtaining exact positions, and the resulting radiants are reliable to within unusually small limits of error. I believe that diffused or elongated radiants, as such, do not really exist, but are due either to errors of observation or to nearly concentric streams of contemporary epoch.

Meteor systems of considerable inclination can only remain in visible activity for a very short time if they describe parabolic orbits, for the Earth's velocity will enable her to traverse a stream of five millions of miles in breadth in about three days.

* The sinuous or curving paths of certain meteoric particles may also be due, as Prof. Newton has suggested, to unsymmetrical forms. The resistance of the atmosphere upon such objects would probably cause a deviation from their original direction of flight.

Enormous breadth will originate longer duration, but the radiant point will show a displacement amongst the stars. Streams which encounter the Earth near the apex will, on the other hand, continue to be displayed for several weeks, according to the circumstances of the *rencontre*, but in every case there will be a gradual change of position in the direction of increasing longitude. *Perseids* from the August shower continue to fall during the twenty-six nights from July 25 to August 19, and their derivative radiant point advances from about R.A. 27° to 68° in that interval, as proved by my observations since 1867. There is, perhaps, no meteor shower whose duration is better ascertained than that of the noted August *Perseids*. The display first assumes a distinct and definite form on the night of July 25, when it furnishes about one meteor per hour. Thence it develops an intensity until August 10, after which it declines somewhat rapidly until August 19, when it appears to have become nearly exhausted, though, I believe, especially from my observations of the present year, that it is feebly sustained until August 22, when it gives about one meteor in three hours within the sphere of vision commanded by one observer. At the end of July the radiant point is close to χ *Persei*, on August 12–13 it has advanced to near B – C *Camelopardi*, and on August 19 is at $68° + 57°$, or $22\frac{1}{2}°$ E. of its place on July 25. On August 22, 1884, I suspected very feeble traces of the shower from $77° + 56\frac{1}{2}°$. I have drawn up the following Table from an analysis of all my observations of this prominent system :—

Date.	Horary No. of all Meteors.	Horary No. of Perseids.	Radiant Point of Perseids.
			α δ
			° °
July 25	20	1	27 + 55
26	22	1	27 + 55
27	29	2	28 + 55
28	29	2	29 + 55
29	26	3	30 + 55
30	24	3	31 + 55
31	21	4	32 + 55
Aug. 1	18	5	33 + 56
2	18	5	34 + 56
3	20	6	35 + 56
4	21	6	36 + 56
5	22	7	38 + 57
6	24	8	39 + 57
7	27	10	40 + 57
8	34	16	42 + 57
9	48	32	43 + 57

Date.		Horary No. of all Meteors.	Horary No. of Perseids.	Radiant Point of Perseids. α δ ° °
Aug.	10	71	57	45 + 57
	11	44	29	47 + 57
	12	26	11	49 + 57
	13	23	7	51 + 57
	14	23	5	54 + 57
	15	22	4	56 + 57
	16	19	3	59 + 57
	17	18	2	62 + 57
	18	18	1	65 + 57
	19	18	1	68 + 57

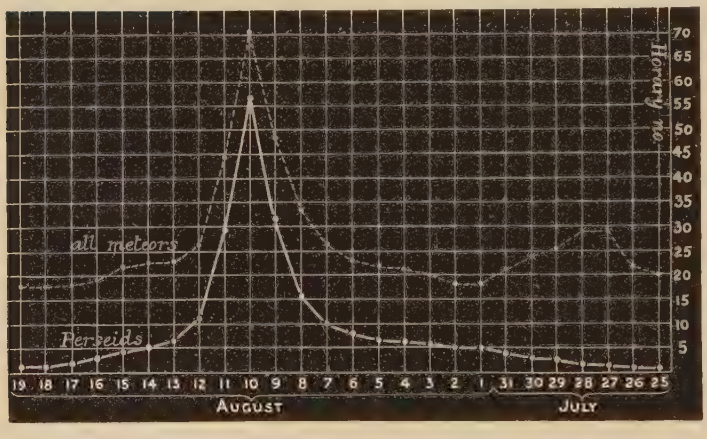


FIG. 1.—Diagram exhibiting horary numbers of all Meteors and Perseids. July 25–Aug. 19.

The horary number of all meteors on July 27–29 is very large, but the dates correspond to the July meteoric epoch, when the *Aquariads* furnish some eight or ten meteors per hour. The number of *Perseids* varies to some extent in different years owing to several circumstances, but I believe the average horary rate for this special stream, and the numbers inclusive of all contemporary showers, will be found near the truth. They are for one observer looking continuously towards the eastern heavens with very clear sky and in the absence of moonlight. At sea the horary numbers will usually be greater than those assigned ; in towns, where mist and smoke interfere, and there is a good deal of artificial illumination of the atmosphere, the numbers will be less. Observations made from an open window, where the view is generally more restricted than out of doors.

will also have the effect of diminishing the horary rate of apparition.

The fact referred to as to the adherence of many showers to definite points during lengthy intervals is one which offers a singular contradiction to prevailing opinions. The peculiarity has been noticed by several observers, and if admitted, as it will most unquestionably have to be sooner or later, will open up some salient points for consideration, and perhaps materially alter our theoretical ideas as to the construction of meteor orbits as well as extend our views as to the effects of planetary perturbations upon these cosmical streams. It undoubtedly merits close attention, and it will be a most gratifying circumstance to find it subjected to a rigorous test by several competent observers. This might be effected without much difficulty by systematic observation undertaken during a few months in several successive years. The only conditions I would impose are: (1) That the observations must be continued for as lengthy a period as possible every clear night; (2) that all such paths as are utilised in the determination of radiants are to be accurately recorded; (3) that in each case the visible features be carefully considered, so that the individual meteors may be arranged in families according to their distinctive appearances, and thus the possibility of false radiants obviated; and (4) that the observations of not more than two successive nights be combined in ascertaining the positions. I make the latter condition because it has been asserted that many pseudo-radiants have resulted from the comparison of meteors registered during several weeks, and that were the observations kept separate and the positions determined for each night, or at most confined to two or three nights, the alleged long duration must disappear as the result of an erroneous method. I venture to assert that the very reverse of this will prove to be the case. The more limited the period for which the radiants are derived, and the more accurately they are fixed, the greater the number and more striking the character of the resulting coincidences. It is to be recommended on several grounds that the observations be grouped into intervals as short as is consistent with satisfactory reductions, and the observer should free himself of all bias as to positions he may have recognised on preceding nights, or which may have been determined by other observers at the same epoch. His main end should be the accurate recording of the directions of meteor flights and the detection of their sharply defined points of intersection. The only objection to the suggested short-period observations is the extreme feebleness of the great majority of showers,* which renders them difficult to identify in a single night, so that observers who limit

* A large proportion of streams give only 1 meteor in 3 or 4 hours. There are probably at least from 70 to 100 showers progressing every night, whereas the horary number of meteors does not usually exceed 15, so that the extreme sparseness of many of these systems is evident.

their reductions to this extent must necessarily watch during the entire night if their results are to be thoroughly full and trustworthy. It is true that very precise radiants are often derived from the intersection of only three or four meteor paths very exactly observed, but it is advisable to obtain as many tracks as possible to prevent the risk of pseudo-radiants. I have seen showers so attenuated as to afford only about one meteor in six hours, and there are a great number of more feeble character than this. Many streams also exist which are apparently of very intermittent nature. Two well-defined observations of a shower may be secured on dates separated by a few intermediate nights on which no conformable meteors are registered. This probably results from an irregular dispersion of the particles along the orbit. Were several observers watching the progress of the shower from widely distant stations it would doubtless appear continuous, and I support this opinion on the fact that in the case of certain radiants which have exhibited intervals of perfect rest one year, have in subsequent years shown great distinctness on the very nights of their former quiescence. Even during the same observation, when a lengthy one, I have sometimes noticed a similar peculiarity in a marked degree. In the first hour or two some half a dozen meteors have displayed sharp radiation and perfectly similar features, but during the middle hours of the watch no further indication of the stream has been remarked; towards the close of the observation, however, some further meteors have become visible from the same source. This proves that an observation extended for, say, three or four hours, is not always sufficient to definitely prove the non-existence of a stream whose visibility at the particular time may happen to be influenced by one of the temporary lulls to which they appear commonly subject.

This interrupted fall of meteors would manifestly often occur, especially in the instance of feeble systems giving, say, one meteor in three or four hours. The heavens abound in examples of such streams, and they are most densely congregated in the region following* the Earth's apex. We cannot assume that meteoric orbits consist of a perfectly regular dispersion of particles: in fact, the visible behaviour of these phenomena, so far as observations have enabled us to comprehend them, is conclusive as to the probable occurrence of sectional derangements (i.e. alternating richness and sparseness of distribution) that affect the local intensity and initiate that variable tendency which is so often the observed effect. Even in the case of exceptionally brilliant meteor storms we have it recorded that temporary lulls have occurred and imparted to them a somewhat

* The region of maximum richness is not *at* the apex, as is commonly understood, but some 40° *following* it (i.e. 130° from the Sun). The largest number of showers congregate here, and the fact is borne out by the relative horary rates of apparition of meteors which reach the highest figure at about 2^h or 3^h A.M.

intermittent character both in their development and decline. If, therefore, such conspicuous displays are sensibly affected in this manner, we can readily admit its operation in regard to minor streams whose chief peculiarities consist in their extreme tenuity of distribution and in the difficulties attendant upon their satisfactory identification.

It is not my intention to give an exhaustive analysis of the showers upon which I rely as demonstrating the long duration of certain radiants. I have selected six positions as examples; they are situated comparatively near together and will admit of conjunctive observation. It will be noticed in reference to each of these displays that they show a marvellous agreement in each of their successive positions. I subjoin tables of my own results and a supplementary list of other observations, together with the mean places derived from them separately and collectively.

The mean positions are as follows:

	Radiant. α	δ	Apparent Duration.	Shower.	Positions averaged.
I.	30°·0	+36°·0	July 16–Nov. 14	β Triangulids	23
II.	46·0	+45·6	6– 30	α–β Perseids	31
III.	61·0	+47·7	25– 27	μ Perseids	21
IV.	61·8	+36·8	Aug. 2–Dec. 31	ϵ Perseids	26
V.	76·2	+32·6	July 23– 27	ι Aurigids	21
VI.	80·2	+22·9	Aug. 24–Jan. 15	ζ Taurids	25

I have conjoined the results of various observers with my own determination in order to show the degree and nature of the corroboration with regard to each shower. But it is upon my own observations that I rely in regard to the long duration of exactly identical radiants, for it must be sufficiently obvious to anyone, who considers the circumstances, that a miscellaneous collection of radiants even of the same shower will exhibit somewhat large discordances owing to difference of method and relative accuracy amongst the observers. On the other hand, a single observer who closely watches the fall of meteors and invariably pursues a uniform method, and whose sole aim is the detection of reliable and exact radiants, will obtain results in far better agreement than is found to exist between the positions given by different persons.

In the following Tables the abbreviations are H., Heis; S., Schmidt; S. Z., Schiaparelli and Zezioli; G. H., Greg and Herschel; A. S. H., A. S. Herschel; W., Weiss; T., Tupman; C., Corder; Sa., Sawyer; L., Lorenzoni; F., Felgel; M., Maggi; Dz., Denza; D., Denning (radiants observed); D.*, Denning (radiants derived from other observations).

α	δ	I. β *Triangulids.*	
°	°		
27	+ 35	July 16–24	D*.
28	36	26–August 1	D.
30	37	29	D.
30	36	August 4	D.
30	35	July 15–September 14	D*.
30	36½	August 25	D.
30	38	21–31	D*.
30	36	September 14, 15, 21, and 25	D.
31	36	October 8	D.
31	37	13–19	D.
29	37	November 4–10	D.
29·6	+ 36·3	July 16–November 10	
32	+ 35	July 30	S. & Z.
33	33	August 5	L.
29	37	12–14	W.
30	40	August–September	C.
28	35	September 23	
36	35	September	G. H.
28	35	September 19–October 15	H.
27	36	October 8	Sa.
28	33	17–22	Sa.
32	34	22–31	C.
27	35	October–November	C.
33	40	November 13–14	Dz.
30·3	+ 35·7	July 30–November 14	

30°·0 + 36°·0 mean of 23.

α	δ	II. α–β *Perseids.*	
°	°		
47	+ 45	July 6–17	D.
48	43	23–25	D.
41	40	25–31 and August 13	D*.
50	47	August 6–12	D*.
44	46	2–11	D*.
49	47	6–12	D*.
46	45	3–16	D.
46	45	19–21	D.
46	47	21–23	D.
45	47	21–31	D*.
44	43	24–September 14	D*.

α	δ		
°	°		
47	+ 45	September 4–16	D.
45	46	October 20	D.
46	46	October 31–November 4	D.
48	43	November 12–14	D.
46·1	+ 45·0	July 6–November 14	
50	+ 48	August 3–12	S.
47½	48	6	T.
42	48	7	S. Z.
48	49	10	M.
47	43	11	S. Z.
40	45	August	H.
48	41	September 5	T.
45	50	August 29	T.
45	50	September	C.
46	48	September 21	C.
45	40	1–13	H.
44	46	October	C.
47	47	October	C.
46	44	October 16–November 30	H.
42	43	November 10	Sa.
50	49	November	G. H.
45·8	+ 46·2	August 3–November 30	

46°·0 + 45°·6 mean of 31.

III. *μ Perseids.*

α	δ		
°	°		
63	+ 50	July 25–31	D*.
61	48	August 3–16	D.
61	48	6–12	D*.
61	50	21–23	D.
60	50	21–31	D*.
63	46	September 1–10	D*.
61	48	14–25	D.
62	46	26–30	D*.
61	47	October 2–18	D.
62	47	20–31	D*.
61	49	28–November 13	D.
59	48	November 12–13	D.
61·3	+ 48·1	July 25–November 13	

α	δ		
°	°		
56	+ 47	August 3–11	S.
61	43	10	P.
63	46	10	J.
60	51	11	A. S. H.
62	48	August	C.
68	46	August 3–30	G. H.
62	46	29	H.
55	45	October 2–November 23	H.
59	53	November 15–27	H.
60·7	+ 47·2	August 3–November 27	

61°·0 + 47°·7 mean of 21.

IV. ϵ *Perseids.*

α	δ		
°			
59	+ 39	August 2–11	D*.
61	39	6–12	D*.
62	35	21–31	D.
62	37	25	D.
67	35	25–September 14	D*.
63	38	September 1–10	D*.
61	36	4–16	D.
60	37	5–12	D*.
61	36	6	D.
63	34	26–30	D*.
62	37	October 20–November 12	D*.
62	36	29– 13	D.
60	34	November 4–12	D.
62	38	1–7	D*.
62	34	12–13	D.
60	37	25–December 13	D.
62	38	25– 31	D*.
62	38	December 1–8	D*.
61·7	+ 36·6	August 2–December 31	

61	+ 39	August 10	
64	39	11–26	H.
60	32	September 6	S. Z.
66	40	7–15	T.
55	37	1–30	H.
62	37	November 6	A. S. H.
61	42	9	S. Z.
66	34	December 3–11	H.
61·9	+ 37·5	August 10–December 11	

61°·8 + 36°·8 mean of 26.

α	δ	V. ι *Aurigids.*	
75°	+ 31°	July 23	D.
74	33	August 6–12	D*.
75	33	27	D.
76	32	September 14–25	D.
76	34	26–30	D*.
77	31	October 8	D.
78	33	12–31	D*.
76	33	14–15	D.
78	37	29–November 13	D*.
77	32	November 7	D.
76½	33	12–13	D.
85	33	20	D.
77	33	20–30	D*.
78	32	December 9–12	D*.
76	34	21–27	D*.
77°·0	+ 32°·9	July 23–December 27	
70	+ 31	August 29	T.
70	32	September	S.
80	30	September	C.
77	37	October 8	T.
77	30	13	T.
71	31	10–27	S.
74°·2	+ 31°·8	August 29–October 27	

76°·2 + 32°·6 mean of 21.

α	δ	VI. ζ *Taurids.*	
78°	+ 23°	August 24–September 14	D*.
81	23	September 5–12	D*.
80	25	14–25	D.
80	21	October 2–November 13	D.
81	23	12–31	D*.
80	24	November 12–14	D.
82	24	20–30	D*.
80	23	22–December 8	D.
78	24	27	D.
80	25	29–December 8	D.
81	23	December 1–8	D*.
83	23	7–13	D.
80	23	January 1–15	D*.
80°·3	+ 23°·4	August 24–January 15	

α °	δ °		
78	+ 23	August 29	T.
78	23	September 8–10	T.
80	19	October 13	S. Z.
78	25½	21	S. Z.
80	22	November 20–December 9	Sa.
81	21	26–29	Sa.
81	23	21–28	Sa.
80	20	December 4–12	G. H.
82	23	7–12	Sa.
78	23	7–13	C.
83	23	11	F.
81	22	27	A. S. H.
80·0	+ 22·3	August 29–December 27	

80°·2 + 22°·9 mean of 25.

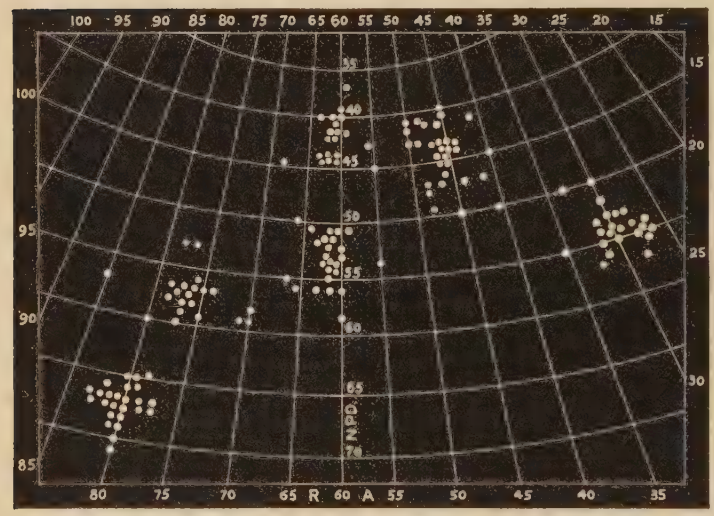


FIG. 2.—Individual positions of showers belonging to long-enduring radiants.

I. A remarkably well-defined radiant point frequently appearing during the five months, July to December. The position is about 1½° N. of *β Trianguli*. The conformable meteors are small; fireballs rarely come in this shower. It has been most fully observed during July–August, owing to the circumstance that at the epochs of the July and August meteors a large number of observers have explored this special quarter of the sky. In October and November the south-westerly position of

the shower late in the night, with its high altitude, would render it more difficult of detection unless by observation in the early evening. Even assuming an equal activity of the radiant in autumn as in the summer, it would have attracted considerably less notice from the causes assigned. The place of divergence, as I have repeatedly observed it in each of the five months of its apparent duration, has been singularly sharp and persistent at the point about 30°+36°. It formed rich displays on July 29, 1879, August 4, and October 8, 1877, and August 25, 1884. My own determinations of the radiant are all nearly coincident. This is hardly applicable to the series of positions derived by other observers, but the comparison of such results by widely different observers must certainly introduce considerable discordances. Even in cases where a prominent periodical shower has been the one observed, a combination of the values for the radiant as given by various astronomers will disclose positions

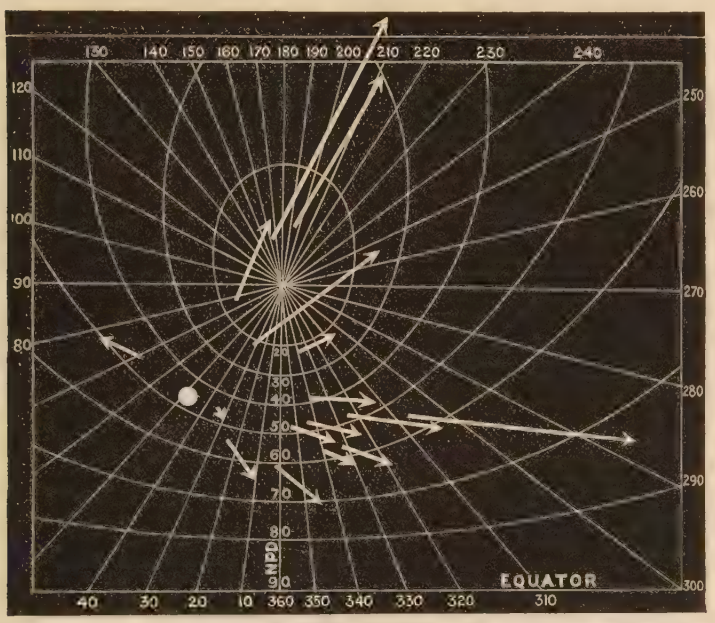


Fig. 3.—Meteor shower from α–β Persei (48°+43°) July 23–25, 1884 (W. F. Denning).

differing to the extent of 5° or more. The mean centre for this shower near β *Trianguli* is, however, very accordant as regards my own results and those of other observers, and I believe the individual determinations are all well within the probable errors of observation. With respect to the averaging of bordering radiants and the deduction of mean positions, I would remark that the work requires great caution and discrimination, owing

to the crowding together of streams perhaps distinct in themselves though nearly similar in some of their elements.

II. The mean position falls almost exactly between α and β *Persei*. My several estimates for this radiant between July and November are nearly identical. I have seen it on many other dates in addition to those mentioned in the list, though not with satisfactory fulness, and as far as my observations enable me to judge, the shower is very nearly continuous as well as stationary during the five months named. In July and August it is particularly rich, and I have often observed its brilliant streak-leaving meteors with great interest. On July 23–25, 1884, it formed a rich shower, and furnished a close agreement with the radiant and date of Messier's Comet of 1764, but the accordance is very doubtful as regards the inference of physical connection. This radiant has been more frequently observed in July–August, because of the great number of observations annually made at this season, and it is situated only 12° S. of the *Perseids* of August 10. Mr. Corder has seen it in striking activity in October and November, and it is marvellous how the very same point of radiation is renewed again and again. This is the significant fact with respect to these α–β *Perseids* and other long-continued showers, that the successive points of departure are very nearly coincident, while the immediately adjacent spaces are devoid of such displays. This centre in *Perseus* is also the seat of activity in January, according to the observations of Heis and Schiaparelli and Zezioli.

III. Close to μ *Persei* and 5° E of δ. This shower has not been so numerously observed as the preceding one, but it exhibits the same remarkable accordance in its successive positions. It has been most satisfactorily distinguished in the months of August and September, when this especial region has been the centre of a large amount of observation. In conjunction with the showers near $\alpha-\beta$ and ϵ *Persei* it forms a triangle, and it is very singular that these points are manifested again and again as the very exact centres of meteor showers. Occasionally fireballs are emergent from this stream at μ *Persei*; the great meteor of August 11, 1876, had a radiant at 60°+51 (A. S. Herschel). The corroborative radiants adduced from the catalogues of other observers are not so nearly coincident as the summary of my own positions, though the mean shows little departure. This shower, though comparatively near the $\alpha-\beta$ and ε *Perseids*, is hardly capable of confusion with them unless the meteors are observed inaccurately, and without due regard to their apparent directions. In cases like this, where it is desirable to separate bordering showers, it becomes more than ever necessary to register the individual paths with the utmost attainable precision, because there is often very little distinction in the visible aspects of meteors proceeding from regions so nearly adjacent.

IV. 5° S.E. of ϵ *Persei*. The radiant is persistent during

the last five months of the year. It forms a very noteworthy shower during the first half of September, especially about the 6–8, and again on about November 4. At these epochs it supplies fireballs of the largest dimensions.* The great bolide of November 6, 1869, $6^h\ 50^m$, observed by myself and many others, had a radiant point at $62°+37$ (A. S. Herschel).† The doubly observed meteor of August 10, 1872, had a radiant at $61°+39°$. This shower is very well marked during the whole period of its duration, and shows a sharply-defined radiant point. In August and September its meteors are very swift, and bright streaks are their characteristic feature. The supplementary table of radiants is not very consistent in the individual agreements, though the mean is almost exactly coincident with my own result. It is rather doubtful whether the position at $61°+42°$, Nov. 9, by S. and Z., belongs to this shower or to the following one near *μ Persei*.

V. This radiant is some 2° following a line connecting *ι Aurigæ* and *β Tauri*, and 3° E. of the former star. The position is very sharply defined, and it represents the focus of divergence of numbers of shooting stars during the last half of the year. The corroboration by other observers is excellent as regards the mean, but the separate values are not as accordant as might be expected, though they undoubtedly represent the same radiant point. It is satisfactory to note, however, that Tupman's position at $77°+30°$, Oct. 13, which is the only one falling really very near the true centre, was specially designated as "accurate" by its careful observer. The pair of positions by Schmidt in September and October close to *ι Aurigæ* are proved both by my own and Tupman's results to be 5° too far W. On the other hand, the position by the latter observer for Aug. 29 is equally discordant in the same direction, and the radiant I saw on Nov. 20 is some 7° E. There are errors in several of the other values, which are obviously too far N., and that these errors are real and capable of being thus assigned I have assured myself by a large number of observations and reductions as accurate and complete as it is possible for me to make them. The radiant is strikingly permanent, and further views of it are much required during its earlier displays, especially in August. The shower is very distinctly visible on about the 27–29th of that month, but it can only be well distinguished in the morning hours, owing to the low position of the radiant in the earlier hours of the night. It occasionally supplies fireballs, for the great meteor, estimated as of equal brightness with the full Moon, seen in the South of France on Nov. 11, 1864, in the evening twilight, had a radiant at $83°+35°$ (± 10). A pair of fireballs observed near midnight on Oct. 8, 1877, also very probably belonged to this stream. Heis appears to have noticed something of it in the months of October–November; for he gives a position at $83°+28°$ (47

* See *Observatory*, September 1882, p. 262–5.

† *Proceedings of the British Meteorological Society*, June 15, 1870.

meteors), which is evidently a combination-radiant resulting from a confusion of the paths from this shower near *ι Aurigæ—β Tauri*, and the bordering one, 10° S., near *ζ Tauri*, which would obviously indicate radiation from the place assigned by Heis.

VI. The mean centre is 3° N.W. of *ζ Tauri*. A succession of showers have their origin here during the last five months of the year, and there are continued traces of radiation from the same point in January. It has been principally distinguished, however, between Nov. 20 and Dec. 13, when it constitutes a display of great distinctness, and the prominent character of the shower has been admitted by several observers. I detected this stream in unusual activity on Dec 6, 1876, and it has re-appeared annually since that time. In the case of this radiant the supplementary positions adduced from the catalogues of other observers show an equal consistency with my own, and there is no doubt as to the prolonged duration. More observations will be very useful, however, in September and October. Tupman found it a very active shower on Sept. 8–10, 1869, and it would doubtless have commanded more observers at this early period but for the fact of the late rising of the radiant point, which necessitates morning observations. I do not think Schmidt's shower at 79°+13°, or Tupman's at 75°+15°, Sept. 22, and 77°+10°, Nov. 10–11, or several positions of my own at nearly the same places are to be regarded as connected with these *ζ Taurids*, as they are fully 10° distant. And it is not advisable to include positions differing to such an extent, as they are evidently distinct systems which must, if combined with systems really associated, only introduce a false mean. Fireballs of the largest type sometimes emerge from this radiant. The fine meteors of Dec. 8, 1861, and Dec. 27, 1864, gave positions at 91°+18° (±10°) and 81°+22° (±15°) respectively. I saw a fireball from this stream on Dec. 6, 1883.

In addition to these selected examples there are many other instances of abnormal duration in radiants retaining a fixed place amongst the constellations. I subjoin a list of some of the most prominent and best observed of these, as they will doubtless be often re-observed in future years:

Ref. No.	α °	δ °			
7	6·6	+12·7	June	to	September.
8	7·2	+52·1	July	„	November.
9	7·7	+35·9	July 21	„	November 13.
10	31·2	+17·8	July	„	November.
11	43·9	+25·1	August 3	„	November 13.
12	69·5	+65·2	July 25	„	December 8.
13	70·0	+50·7	August 21	„	November 4.
14	76·2	+44·8	August 4	„	December 14.
15	78·0	+75·7	August	„	October.

Ref. No.	α	δ			
	°	°			
16	9·1	+55·8	August 6	to	December 31.
17	97·4	+44·3	August 6	,,	December 8.
18	106·1	+11·9	Sept. 15	,,	January.
19	108·5	+24·2	September	,,	December
20	132·3	+46·9	September	,,	February.
21	132·5	+20·8	October 11	,,	February 16.
22	145·4	+ 3·9	November	,,	April.
23	181·1	+34·7	October 16	,,	April 12.
24	206·1	− 7·6	January	,,	May.
25	226 6	− 5·4	January	,,	May.
26	268·6	+23·4	March	,,	September.
27	285·9	−13·0	Feb. 10	,,	August.
28	290·6	+69·7	July 26	,,	October 31.
29	315·5	+60·0	April 19	,,	November 15.
30	332·2	+36·1	March 17	,,	September.
31	332·8	+48 6	June	,,	October.
32	334·8	+27·2	May	,,	September 14.

Stationary, long-continued radiants appear to be a general feature of attenuated showers, though I have singled out some equally well-defined displays of very brief, fugitive character, which are doubtless of similar type to the *Quadrantids* of Jan. 2 and *Lyrids* of April 20. There are many varieties apparent amongst these phenomena, not only in the individual meteors, but in their congregated streams. The permanent showers appear to occur irrespective of inclination. The first decided intimation of their presence is usually recognised when the radiants are near the Earth's apex. At such times they furnish very swift streak-leaving meteors. Later on they lose the capacity to generate streaks, and ultimately are transformed into the slow train-bearing meteors whose radiants cluster in regions far removed from the Earth's direction of motion. Yet during the whole time of the display, and while the individual meteors are thus visibly affected by the change, progressing from night to night in the positions of their divergent points relatively to the Earth's apex, their radiants remain immovable; and the fact is conclusively proved, not by approximate accordances, but by absolute coincidence in these points as observed with great care and precision.

Since 1877 I have generally confined my radiants to short periods, though in some cases have been compelled to associate many showers whose positions were almost identical. Thus, the *Andromedes*, which maintain an apparently continuous display between July and December, were found to be so frequently visible that they gave about forty separate radiants, all agreeing

fairly well. To clearly exhibit my meaning, I add a summary of my position for this stream during the months of July and August as resulting from my own observations. Had I dissected

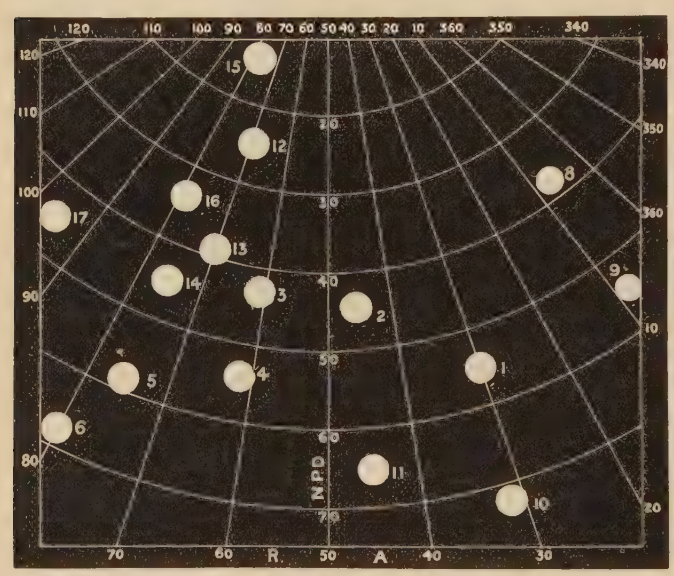


FIG. 4.—Positions of 16 long-enduring radiant points.

my reductions of meteors recorded by foreign astronomers, the number of positions would be greatly augmented:—

	α	δ		α	δ
	°	°		°	°
July 7	4	+35	August 3–4	9	+34
11	5	+35	7	8	+38
12	7	+37	10	6	+37
16	5	+35	11	8	+38
26	3	+35	12	10	+38
27	6	+34	16	10	+37
28	7	+36	20	3	+35
29	7	+37	21	5	+37
30	6	+35			

Mean of 17 (July–August) = 6°·4 + 36°·1.

Analysing the six special showers near *β Trianguli*, *α–β*, *μ*, and *ε Persei*, *ι Aurigæ*, and *ζ Tauri*, I find a numerous series of positions showing even closer agreements than in the case of these *Andromedes*. The radiant at 30° + 36° I have seen with great cer-

tainty and definiteness of radiation on twenty-one different nights, and it always recurs from very nearly the same point. Such trivial differences as are apparent are solely due to unavoidable errors of observation. My best determinations more nearly coincide than those which I regard as less certain. These showers appear to be continuous as well as stationary, for the accumulation of new observations has the tendency to fill up the epochs of their apparent quiescence.

In the instance of very feeble showers it is dangerous to watch for them particularly, as a few meteors will certainly be recorded with directions conforming to their radiant points, though really belonging to other systems. I believe, however, that an observer of experience, who is accustomed to grouping the showers according to their traits of appearance, will not be likely to introduce errors of this kind, though it is always safest to register the meteors in the ordinary manner, and then deduce the most certain radiants, without regard to expected showers or to positions formerly assigned by other observers on the same dates. This is the only method of obtaining perfectly independent and reliable centres eligible for comparison with the similar investigations of preceding years.

I have mentioned the fact of this seeming continuity of meteor streams for several reasons. Though frequent revivals of identical radiants may not essentially mean a prolonged exhibition of the same meteor orbit (it is impossible to comprehend how it can be interpreted in that way with due regard to prevailing ideas as to the forms of meteo-cometic orbits), it yet strikingly suggests some curious arrangement which it will be very interesting and important to further investigate. A law may be discovered affecting the construction of these streams, or some influence recognised, as the result of such inquiries, which may enable us to adopt the thesis that these alternating showers are connected in some way, and not a mere effect of great numbers causing their re-apparitions from the same points. If this explanation were applied, there would be discordances, furnishing a direct negative to the concise and exact character of the radiation which I have in many well-defined instances recognised for several months. And I apprehend there is a meaning attaching to these stationary radiants which we should endeavour to elucidate rather than avoid by apparently feasible explanations, and which, though not altering the fact, must operate to check the renewed observation and analysis by which alone the difficulty admits of settlement.

Another point that has led me into this question is, that we now require a new general catalogue of meteor showers, but it is impossible to satisfactorily arrange the observations of many different observers until this matter affecting the visible duration is finally disposed of. Mr. Greg's "General Comparative Table of Radiant Points and Durations of Meteor Showers visible in the Northern Hemisphere" was published in the *British Association*

Report for 1876, and consisted of 206 mean positions, resulting from a discussion of 850 radiants and sub-radiants founded on some 15,000 shooting stars. Now, since this catalogue appeared we have many additional lists of observed meteor showers by Heis, Konkoly, Weiss, Corder, Sawyer, myself, and others, so that in the aggregate there are more than 2,100 radiants resulting from the projected paths of upwards of 62,000 meteors ! Many of these radiants undoubtedly consist of duplicate observations of identical showers, and I do not believe that the total number of well-defined streams would exceed 350 by a careful comparison and averaging of the positions on the method adopted by Mr. Greg. It is, however, evident that a great necessity exists for a new and thoroughly reliable reference catalogue in which all the hitherto published lists are suitably represented ; and it had been my intention to compile such a work, but the anomalies as to the durations of certain systems confronted me with such force that I was led to abandon the undertaking for the present. The question is, are these radiants to be grouped into periods of several months, or are they to be limited to as many days ? If the former, then the number of showers understood as separate will obviously be greatly lessened ; if the latter, the total will be correspondingly increased. Are we, say, in the instance of the display near μ *Andromedæ*, to dissect it into thirty independent systems, averaging a duration of five days each, or must we present it as one nearly continuous stream covering five months ? While the indubitable evidence of our eyes favours the latter proposition, the equally conclusive teachings of the theory of the subject (which it was thought would so readily accommodate the visible phenomena of meteors, but which has unquestionably given rise to a too hasty generalisation) can only directly oppose such a view.

There is no doubt that this question of persistency is a very complicated one, and I cannot see how it is practical to dissociate the displays into short periods. It will not do to force the observations into a groove with which they do not harmonise. Even if all the results which have accumulated up to the present time were absolutely abandoned as too uncertain and discordant for safe acceptance, and were the individual meteor paths in all available catalogues carefully re-examined and projected with a view to determine the definite radiants for each night separately, the conditions of the case would not be improved. There would be a general blending of exact and wild observations to which would be given equivalent weight. There would be the impossibility to consider those most suggestive details as to the visible aspect of the meteors, such as the length of path as affected by the altitude of the radiant above the observer's horizon, occurrence of streaks or trains, slow or swift motions, all depending on the position of the radiant relatively to the Earth's apex, which must conspire to destroy that very accuracy which would be the main, if not indeed the sole, object of such an investigation. Could a work of this magnitude be conducted

on favourable lines, the results must still exhibit the remarkable permanency of many showers and the difficulty of separating them. For my own part I am firmly persuaded that radiant points, to be thoroughly precise, should be determined by the actual observer of the meteors, who must feel his way very cautiously and with a due regard to all the circumstances which alone can lead him to trustworthy results. The storing up of many thousands of unreduced meteor flights is, therefore, to be deplored. The accurate and safe discussion of such materials is questionable; it could only have been effected at the time by those employed in recording them. I have myself projected some 19,000 meteors in the catalogues of various continental astronomers, and though the results generally corroborate what I have myself seen, I cannot feel nearly the same confidence in them. Look at the tables of radiants by Tupman, Sawyer, and others, who reduced their own observations while fresh in the memory, and there will be found a marvellous number of accurate positions (I say this advisedly after many opportunities of fully testing them), though in some instances they rest on comparatively slender materials. In some other catalogues the most obvious blunders have been committed, and the result is many fictitious radiants. Cases exist in which meteors of 10°, 15°, or 20° in length are assigned to showers a very short distance in the backward prolongation of flight, which is utterly inconsistent with the palpable effects of perspective in apparently contracting the paths close to their originative point. It is most essential that before an observer undertakes the reduction of such observations, he should watch the divergence of meteors from prominent showers like the *Perseids* and *Leonids*, and form some practical idea as to the foreshortening of the tracks which fringe the radiant; but it is not my aim to refer to special misconceptions in a department which does not admit of absolute accuracy, and exhibits anomalies that are even more difficult to explain than they have been to discover.

The prolonged duration of meteor showers gives rise to a curious antagonism of theory and observation which, I trust, will now receive further investigation. The data upon which the discordance rests is, I feel convinced, thoroughly reliable, and renewed observation can only tend to endorse the result arrived at. I am unable to offer any hypothesis in explanation; the matter presents itself as one of great difficulty, but is yet so singularly well marked that it will bear the most critical examination. Though meteor streams like the *Quadrantids* and *Lyrids* are marvellously transient in their visible aspect, the shifting radiant of the *Perseids* shows a more extended, though very distinct, duration of twenty-six days, and the number of systems far exceeding this period are extremely numerous. It will be advisable to select a few of these showers as examples, and rigorously investigate them. Some important peculiarities of arrangement may regulate such long-enduring streams, and we

may find a certain physical association existing among them notwithstanding their dissimilarity of orbit. The most significant fact in connection with this subject is that certain sharply-defined points exhibit a numerous retinue of showers, while the spaces immediately adjacent are comparatively barren of such displays. The radiant points coincide at particular centres, to the marked exclusion of closely-bordering regions. To re-observe with the utmost fulness and accuracy the evidence which the sky affords, leading to this remarkable conclusion, and to discover, if possible, the *meaning* of this singular persistency of showers, will be the important aim of future observations; it may clear away a difficulty from observers, and perhaps enlarge our views as to the visible character of meteor systems. The uninterrupted appearance of shooting-stars in the nocturnal sky offers the ready means of attacking the problem anew and removing any doubts which may still exist as to the stationary, long-enduring aspect of many showers, which must, indeed, remain an indelible effect of all full and trustworthy observation.

Bristol: 1884, *October*.

Observations of Comets Pons-Brooks and Ross. By A. B. Biggs.

(*Communicated by the Secretaries.*)

(*By triangular-bar Micrometer. Dark field.*)

Date.	Mean Time. (Launceston.)	Diff. R.A. Comet from Star.	Diff. Decl.	Name of Star.	Hour Angle. (Approx.)
1884.	h m	m s	′ ″		h m
Jan. 26	9 52 0	+ 1 33·25	+36 58·6	7 Ceti	
	9 56 0	+ 1 36	+37 9·4	,,	
29	8 52 0	− 2 51·5	−22 30	106 Lacaille	5 2 W.
	8 57 0	− 2 49·7	−21 59	,,	5 8
	9 4 10	− 2 47·5	−21 0·8	,,	5 15
Feb. 11	8 45 23	− 0 19·3	+28 59	305 Lacaille	5 9½
	8 55 21	− 0 18·3	+29 36	,,	5 19½
23	10 10 0	+ 3 15·7	+28 56	γ Phenicis	6 54½
	10 16 45	+ 3 16·5	+29 19·6	,,	7 0
Mar. 2	9 53 0	− 3 30	−12 57	542 Lacaille	7 0
11	8 17 30	+ 8 32·7	−17 41	588 ,,	5 35
12	7 35 30	−10 5·5	+ 9 27	693 ,,	4 53
13	8 13 0	− 7 15·5	+39 57	,,	5 34
	8 28 0	− 7 18·5	+40 43·5	,,	5 53½

Date. 1884.	Mean Time. (Launceston.) h m	Diff. R.A. Comet from Star. m s	Diff. Decl. ′ ″	Name of Star.	Hour Angle. (Approx.) h m
Apr. 3	8 48 45	− 0 4	−11 36	989 Lacaille	6 38
	8 57 45	− 0 3·3	−10 1	„	6 47
4	8 50 20	+ 3 33	+10 15	„	6 29
	8 59 20	+ 3 45·5	+10 34	„	6 48
		Comet "Ross."			
Feb. 1	9 15 0	+ 0 14·7	+19 43	9623 Lacaille	6 14
	9 25 0	+ 0 17	+18 24	„	6 24

A very hazy object; nebulous; measures very difficult; no definite point. The only opportunity afforded for obtaining measures.

In all the above measures, different refraction and the comet's proper motion are not reckoned for.

Launceston, Tasmania: 1884.

Ephemeris for Finding the Positions of the Satellites of Uranus, 1885.
By A. Marth.

The angle of position P of the minor axes, the major and minor semi-axes a and b of the apparent ellipses described by the satellites, the longitudes u—U of the satellites reckoned in their orbits from the points which are in superior conjunction with the planet's centre and the planeto-centric latitude of the Earth above the assumed plane of the orbits, are approximately the following:

		Ariel.				*Umbriel.*			
Greenw. noon 1885.	P.	a_1	b_1	u_1−U	Diff.	a_2	b_2	u_2−U	Diff.
	°	″	″	°	°	″	″	°	°
Jan. 13	285·45	14·75	+4·29	114·70	1428·46	20·54	+5·97	134·01	868·75
23	·46	14·87	4·30	103·16	·44	20·72	5·99	282 76	·72
Feb. 2	·47	14·99	4·28	91·60	·41	20 89	5·97	71·48	·70
12	·48	15·09	4·24	80·01	·38	21·03	5·91	220·18.	·69
22	·50	15·18	4·18	68·39	·35	21·14	5·83	8·87	·66
Mar. 4	·51	15·24	4·10	56·74	·33	21·23	5·71	157·53	·65
14	·53	15·27	4·00	45·07	·30	21·27	5 58	306·18	·63
24	285·54	15·28	+3·89	33·37	·27	21 28	+5·43	94·81	·61
Apr. 3	·55	15·26	3·78	21·64	·26	21·26	5·27	243·42	·61
13	·56	15·21	3·67	9·90	·24	21·19	5·11	32 03	60
23	·57	15·14	3·56	358·14	·23	21·10	4·96	180·63	·60
May 3	·57	15·05	3·46	346·37	·23	20·97	4·82	329·23	·60
13	·57	14·95	3·37	334·60	·22	20·82	4·70	117·83	·60
23	·58	14·83	3·31	322·82	1428 22	20·66	4 61	266·43	868·61
June 2	285·57	4·70	+3·26	311·04		20·48	+4·54	55·04	

		Titania.					*Oberon.*			
	Lat. of Earth.	a_3	b_3	u_3-U	Diff.		a_4	b_4	u_4-U	Diff.
1885	°	"	"	°	°		"	"	°	°
Jan. 13	+16·90	33·70	+9·79	207·38			45·06	+13·10	35·30	
					413·53					267·40
23	16·79	33·99	9·82	260·91			45·45	13·13	302·70	
					·51					·38
Feb. 2	16·60	34·26	9·79	314·42			45·81	13·09	210·08	
					·50					·37
12	16·33	34·49	9·70	7·92			46·13	12·97	117·45	
					·48					·36
22	16 00	34·68	9·56	61·40			46·38	12·78	24·81	
					·47					·34
Mar. 4	15·61	34·82	9·37	114·87			46·56	12·53	292·15	
					·46					·34
14	15·20	34·89	9·15	168·33			46·66	12·23	199·49	
					·45					·34
24	+14·77	34·91	8·90	221·78			46·68	11·90	106·83	
					·45					·33
Apr. 3	14·35	34·86	8·64	275·23			46·62	11·55	14·16	
					·45					·33
13	13·95	34·76	8·38	328·68			46·48	11·20	281·49	
					·44					·33
23	13·59	34·60	8·13	22·12			46·27	10 87	188·82	
					·45					·34
May 3	13·29	34·40	7·90	75·57			46·00	10·57	96·16	
					·46					·35
13	13·05	34·15	7·71	129·03			45·67	10·31	3·51	
					·46					·36
23	12·89	33·88	7·56	182·49			45·31	10·11	270·87	
					413·48					267·37
June 2	+12·82	33·59	+7·45	235·97			44·92	+ 9·97	178·24	

These values are to be interpolated for the times for which the positions of the satellites are required. The position-angles p and distances s are then to be found by means of the formulæ:

$$s \sin (p-P) = a \sin (u-U)$$
$$s \cos (p-P) = b \cos (u-U)$$

The satellites move in the direction of *in*creasing position-angles, and will be at their greatest elongations ("N" in posit. P+90° and "s" in posit. P−90°), and at their superior and inferior conjunctions with the centre of the planet about the following Greenwich mean times:

Ariel.

	N.	S.		N.	S.		N.	S.
1885.	h	h		h	h		h	h
Jan. 12	19·9	14 2·1	Mar. 1	17·1	2 23·3	Apr. 18	14·4	19 20·7
15	8·3	16 14·6	4	5·6	5 11·8	21	2·9	22 9·2
17	20·8	19 3·1	6	18·1	8 0·3	23	15·4	24 21·7
20	9·3	21 15·5	9	6·6	10 12·8	26	3·9	27 10·2
22	21·8	24 4 0	11	19·1	13 1·3	28	16·4	29 22·7
25	10·3	26 16·5	14	7·5	15 13·8	May 1	4·9	2 11·2
27	22·8	29 5·0	16	20·0	18 2·3	3	17·4	4 23·7
30	11·2	31 17·5	19	8·5	20 14·8	6	5·9	7 12·2
Feb. 1	23·7	3 6·0	21	21·0	23 3·3	8	18·4	10 0·6
4	12·2	5 18·5	24	9·5	25 15·8	11	6·9	12 13·1
7	0·7	8 6·9	26	22 0	28 4·2	13	19·4	15 1·6
9	13·2	10 19·4	29	10·5	30 16·7	16	7·9	17 14·1

1885.	N. h	S. h		N. h	S. h		N. h	S. h
Feb. 12	1·7	13 7·9	Mar. 31	23·0	A. 2 5·2	May 18	20·4	20 2·6
14	14·2	15 20·4	Apr. 3	11·5	4 17·7	21	8·9	22 15·1
17	2·7	18 8·9	6	0·0	7 6·2	23	21·4	25 3·6
19	15·1	20 21·4	8	12·5	9 18·7	26	9·9	27 16·1
22	3·6	23 9·9	11	1·0	12 7·2	28	22·4	30 4·6
24	16·1	25 22·4	13	13·5	14 19·7	31	10·9	J. 1 17·1
27	4·6	28 10·8	16	2·0	17 8·2	June 2	23·4	4 5·6

Umbriel.

1885.	N. h	S. h		N. h	S. h		N. h	S. h
Jan. 12	11·8	14 13·6	Mar. 3	5·3	5 7·1	Apr. 21	23·0	24 0·7
16	15·3	18 17·0	7	8·8	9 10·5	26	2·4	28 4·2
20	18·7	22 20·5	11	12·3	13 14·0	30	5·9	M. 2 7·6
24	22·2	26 23·9	15	15·7	17 17·5	May 4	9·4	6 11·0
29	1·7	31 3·4	19	19·2	21 20·9	8	12·8	10 14·6
Feb. 2	5·1	4 6·8	23	22·7	26 0·4	12	16·3	14 18·0
6	8·6	8 10·2	28	2·1	30 3·9	16	19·8	18 21·5
10	12·0	12 13·8	Apr. 1	5·6	3 7·3	20	23·2	23 1·0
14	15·5	16 17·2	5	9·1	7 10·8	25	2·7	27 4·5
18	19·0	20 20·7	9	12·5	11 14·3	29	6·2	31 7·9
22	22·4	25 0·1	13	16·0	15 17·7	June 2	9·7	4 11·4
27	1·9	M. 1 3·6	17	19·5	19 21·2	6	13·1	8 14·9

Titania.

Super. Conj. h	N. Elong. h	Infer. Conj. h	S. Elong. h
	Jan. 10 3·8	Jan. 12 8·1	Jan. 14 12·3
Jan. 16 16·6	18 20·8	21 1·0	23 5·3
25 9·5	27 13·7	29 18·0	31 22·2
Feb. 3 2·5	Feb. 5 6·7	Feb. 7 10·9	Feb. 9 15·2
11 19·4	13 23·6	16 3·9	18 8·1
20 12·4	22 16·6	24 20·8	27 1·1
Mar. 1 5·3	Mar. 3 9·6	Mar. 5 13·8	Mar. 7 18·0
9 22·3	12 2·5	14 6·8	16 11·0
18 15·3	20 19·4	22 23·7	25 4·0
27 8·2	29 12·5	31 16·7	Apr. 2 21·0
Apr. 5 1·2	Apr. 7 5·4	Apr. 9 9·7	11 13·9
13 18·2	15 22·4	18 2·7	20 6·9
22 11·2	24 15·4	26 19·6	28 23·9
May 1 4·1	May 3 8·4	May 5 12·6	May 7 16·9
9 21·1	12 1·3	14 5·6	16 9·8
18 14·1	20 18·3	22 22·6	25 2·8
27 7·0	29 11·3	31 15·5	June 2 19·8

Oberon.

Super. Conj.		N. Elong.		Infer. Conj.		S. Elong.	
	h		h		h		h
Jan. 11	16·3	Jan. 15	1·1	Jan. 18	9·9	Jan. 21	18·6
25	3·4	28	12·2	31	21·0	Feb. 4	5·8
Feb. 7	14·6	Feb. 10	23·4	Feb. 14	8·1	17	16·9
21	1·7	24	10·5	27	19·3	Mar. 3	4·1
Mar. 6	12·9	Mar. 9	21·7	Mar. 13	6·5	16	15·3
20	0·1	23	8·9	26	17·7	30	2·5
Apr. 2	11·3	Apr. 5	20·1	Apr. 9	4·9	Apr. 12	13·7
15	22·5	19	7·3	22	16·1	26	0·9
29	9·7	May 2	18·5	May 6	3·3	May 9	12·0
May 12	20·8	16	5·6	19	14·4	22	23·2
26	8·0	29	16·8	June 2	1·6	June 5	9·8